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THE

TEA QUARTERLY

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OF THE

TEA RESEARCH INSTITUTE

OF CEYLON

Edited by

ROLAND V. NORRIS, D.Sc., F.I.C.,

Director, Tea Research Institute.

THE TEA RESEARCH INSTITUTE,

St. Coombs, Talawakelle.

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The Tea Research Institute of Ceylon.

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NOTE.

The Laboratories of the Institute are situated at St. Coombs Estate, Talawakelle, and letters and enquiries should be addressed to the Director, Tea Research Institute of Ceylon, St. Coombs, Talawakelle. Telegraphic Address :—Research, Talawakelle; Telephone, Talawakelle 44 (Private Exchange). It is particularly requested that letters should not be addressed to officers by name.

EDITORIAL.

GREVILLEAS.

Attention has previously been called to a leaf disease of Grevilleas believed to be caused by a species of *Phyllosticta*. This disease was originally confined almost entirely to elevations below 2,000 feet and it was hoped that attacks would not spread to estates up-country. More recently reports have been received indicating that this optimism may be ill-founded. Casualties however occur amongst Grevilleas up-country from other causes than *Phyllosticta*, and as the Institute is most anxious to obtain accurate information in regard to the incidence of the disease, it is particularly requested that any estate on which the disease is believed to occur will send in specimens of leaves for examination Such specimens should preferably consist of freshly fallen leaves obtained by shaking the tree. Examination of such material will enable the Institute to decide whether the disease is really occurring up-country to any marked extent or whether casualities are due to some other cause.

It had been suggested that as an alternative to the Grevillea at lower elevations, the Bead Tree (*Adenanthera pavorina*, Sin. *Mad-eta*) might be employed for shade. The latter tree has been grown in Ceylon for many years and it is believed to be relatively free from liability to disease. We have received one or two reports, however, which suggest that this tree may not be generally suitable. Thus one report states that the wood is hard and does not lop well and that the foliage is normally too dense for high shade. It is also stated that at seeding time and during the dry weather the branches are bare of leaves and that new growth takes a long time to come on. If these comments represent the general behaviour of the tree, it will probably not prove suitable as a substitute for Grevillea and we should be glad to receive further information from others who may have experience of the tree.

CROTALARIAS.

The liability of Tephrosia to eelworm attack has led to a considerable increase in the use of alternative green manure plants, Crotalarias being perhaps the most common substitute. The latter, however, particularly in wetter areas, seem somewhat liable to attack by leaf diseases such for example as that caused by *Ceratophorum setosum* already described in Bulletin No. 13, Annual Report for 1935, pages 31-33. Certain capsid bugs may also cause considerable damage.

The Institute will be glad to obtain further information in regard to the results obtained with Crotalarias in different districts with particular reference to the incidence of diseases and pests, yields of loppings, etc.

MANUAL OF WEEDS.

In the search for ground covers suitable for different areas, a considerable amount of information is gradually accumulating in regard to the suitability or otherwise of many locally occurring weeds. In order that such information may be more readily available and the identification of many of these species facilitated, the Department of Agriculture, with the co-operation of the three Research Institutes, has in course of preparation an illustrated guide to common weeds of economic importance. The manual will consist of a series of coloured plates of the more important weeds with a short non-technical description of the habits of each and notes on their usefulness or otherwise on estates.

The manual when issued will contain 50 such plates and descriptions bound in a loose-leaf binder. A further 50 plates will appear subsequently and be supplied to subscribers. It is hoped that the cost of the publication will not exceed Rs. 3-50.

A sample plate will be issued at an early date to indicate the general scope of the publication.

HELOPELTIS.

Though Helopeltis is not a serious pest of tea in Ceylon and is confined to a relatively small area, it constantly recurs in these districts. It may, therefore, be of interest to refer to recent experiments carried out against this pest in South India. In the report for 1937-38 of the Entomologist of the U.P.A.S.I. Experimental Station, full details are given of extensive trials with various sprays and dusts. Amongst the former Fish-oil insecticidal soap, lime-sulphur, H-G. ionised insecticide and bar-soap solutions have all been employed. The general conclusion reached is that the degree of success obtained was not commensurate with the expense involved. The major problem is the mechanical difficulty of effectively reaching the under side of the leaves. In consequence many of the insects remain untouched and the beneficial effects of the spray are of little permanence.

The results obtained with dusts, which included "Helopelticide." Nico-sect and Pyrethrum, were rather more successful. Here again, however, the conclusion reached is that until further improvements in technique can be devised the process is still too expensive to be considered a practical proposition.

In Ceylon Helopeltis attacks are chiefly confined to low jâts and the only radical cure that seems likely to bring about any permanent improvement is the gradual replacement of these by other jâts.

THE GIANT SNAIL.

The Giant Snail, *Achatina fulica* Fer., is a well-known Ceylon pest which feeds voraciously on many types of crops. It is therefore of some economic importance. Recently successful results against this pest have been obtained by means of a bait composed of "Meta" and rice bran. "Meta" is the well-known solid fuel frequently used for heating small domestic appliances and is sold in the form of packets of 50 or 100 briquettes each about 2 inches by $\frac{1}{2}$ inch by $\frac{1}{4}$ inch. Chemically it consists of polymerised aldehyde.

The bait is made by mixing "Meta" and bran, the former being used in the proportion of $\frac{1}{2}$ oz. to each pound of bran. The mixture is made into a thick mash by the addition of a little water and either distributed broadcast or placed in small heaps of a desert-spoonful at frequent intervals. The bait has given very successful results in Malaya where trials have been carried out by F. Beeley of the Rubber Research Institute of Malaya. A full report of these is published in *The Journal of the Rubber Research Institute of Malaya*, Vol. 8, January, 1938.

The material is apparently not a virulent poison to man or the ordinary domestic animals but care should be taken to keep it out of the reach of children.

We understand that the cost of "Meta" in Ceylon is about Rs. 2 per pound. Full details can however be obtained from the local agents, Messrs. A. Baur & Co., Ltd.

FIELD EXPERIMENTS.

In the present number will be found a reprint of an article entitled "The Uses of Statistics in Field Experiments with Rubber" by the Director of the Rubber Research Institute of Malaya. Considering the title, some explanation might seem necessary for its inclusion in a Journal confined to Tea. In fact this is not the case and we believe that this paper will be read with much interest by tea planters. The layman often finds some difficulty in appreciating the general principles underlying the design of modern field experiments and the interpretation of the results obtained. Mr. Page's paper explains in very clear language why, in general, single plot experiments cannot be relied on to give information of

value, and illustrates with many straightforward examples the necessity for the more complex comparisons which the scientist now adopts. The fact that the actual examples quoted relate to rubber in no way diminishes the application of the paper to experiments with tea. It seems sometimes to be considered that a scientist chooses these more complex experimental methods out of sheer perversity! A reference to previous articles in the Institute's earlier publications (T. Eden, "The Aim of Field Experiments," *Tea Quarterly*, 1928, Vol. I, pages 36 and 60) and a study of Mr. Page's paper, will perhaps help to remove this illusion and persuade sceptics that the unfortunate scientist has in reality little or no choice in the matter.

TEA MACHINERY

At present brass, zinc and galvanised iron are widely employed in the construction of component parts of tea machinery. All these materials have certain disadvantages however, and it is likely that in the future they will be largely replaced by other metals or alloys. Progress has no doubt been delayed by the higher cost of most of the possible alternatives but we would invite the attention of engineering firms interested in tea machinery to the desirability of greater consideration being given to this question.

Aluminium is gradually finding a greater use for containers, fermentation trays and, as stamped mesh, for dry tea sifters. For components requiring greater strength stainless steel is suitable and would no doubt be widely adopted but for considerations of initial cost.

It is suggested that the nickel-copper alloys, of which Monel metal is an example, might be examined for their suitability in tea manufacture. These now find a wide use in food production plants in many countries. The Institute hopes shortly to have an opportunity for carrying out tests on some of these materials which from their strength and resisting powers to erosion would seem likely to meet the requirements of the tea industry.

TARING OF TEA CHESTS.

Shippers to the London market often wish to bring their tea chests to an even tare. It is suggested a convenient method of doing this would be the use of short lengths of strip iron nailed to the inside of the lid. Such strips, about 1 inch wide and $\frac{1}{8}$ inch thick, can be obtained, cut in standard lengths of definite weight, *e.g.*, $\frac{1}{2}$, 1, 2, 4 ozs., etc., and punched ready for nailing. The cost is about Rs. 15 per cwt. Ordinary tin tacks are the best form of nails. These should be long enough to penetrate the lid and be bent over on the upper side.

To obviate risk of damage to the lining of the chest it may be desirable to protect the strips with paper. This point is receiving attention.

PUBLICATIONS OF THE INDIAN TEA ASSOCIATION, SCIENTIFIC DEPARTMENT, TOCKLAI.

We are advised that the following publications issued by the Tocklai Experimental Station can be obtained on application to the Assistant Secretary, Indian Tea Association, Royal Exchange, Calcutta.

1. Memorandum No. 1, The Occurrence and Treatment of Bark-Eating Caterpillars and Borers. Price Re. 1.
2. Proceedings of the Second Annual Conference between representatives of the planting districts in North-East India and Members of the Staff of the Tocklai Experimental Station. Price 6 Annas.

ROLAND V. NORRIS.

REPLANTING—II.*

TEA SELECTION

F. R. TUBBS

An important question that has to be faced by the Ceylon tea industry in the immediate future is whether planned programmes of replanting are to be undertaken, or whether we are to remain content to continue to live on and in the past rather than to make any contribution to the future. We are often content to judge our efforts in comparison with those of our immediate neighbours, but hard economic facts have nowadays developed the habit of breaking through the barriers raised by insular custom. A short time ago the Institute drew attention to the subject of replanting and pointed out that the progress being made in other tea-producing countries demanded, at the very least, that we give it our earnest consideration. It was not attempted to provide a general answer to the question — that has to be decided in the light of conditions individual to each estate and is therefore no concern of ours today. But the issue of replanting having been raised, the cognate one of what we are to replant with must also receive our careful consideration; in other words, what efforts are we willing to make to ensure that replanting (and for that matter, supplying) is only done with material that is an improvement on what has been used in the past? To achieve such improvement, selection of the better from the poorer is an essential process. That is what tea selection denotes — the elimination of the relatively poorer types.

We will all agree, I think, that such a process is desirable, but before it is put into practice, the more critical may ask what evidence there is that it is necessary, what is involved in carrying it out, and what improvements may be reasonably hoped for. These questions form, however, far too large a subject for a brief talk, and only an outline can be given today.

What evidence have we that sufficient inherited variation occurs in tea to justify selection by the planter in the course of the management of the estate?

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* Lecture given to the Dimbula District Planters' Association on the 30th May, 1938.

In Ceylon we are planting the descendants, relatively at random, of wild tea bushes of varying types, and their progeny by random crossing, and, as a natural result, visible variation is only too obvious. In bush type and leaf type we have easily observed characters and the variability observable in this respect in tea exhibits its full range around us! I have in one plot forms that appear to be unable to grow more than nine inches high, with flush that is relatively microscopic in size, growing alongside large, high-jât bushes. So, I know, have you! While the former may, for all we know at present, be as estimable as the latter in some respects, the juxtaposition of the two in a field can only result in inefficient working. Invariably, "China" jât fields, for example, receive different treatment to those of "high" jât tea, to take the extreme case. Uniformity of bush type within a field will enable us to adapt the treatment to the requirements of the individual bushes as well as to those of whole fields, instead of being forced to apply a treatment suitable to the average, leaving those bushes which vary from the average to suffer for their temerity. Take the question of pruning cycle alone; some bushes will run eighteen months, some four years; so they are lumped together at three.

Turning from easily visible "jât" characters to yield, we find that critical evidence of the range of inherited variability in yield is as yet lacking, and for this we must await the day, not too far distant I hope, when the variability within a clone due to very localised differences in bush environment, can be compared with that within a seedling population. The variation in yield from bush to bush in a field is a function both of the inherited internal environment and the chance external one, and it is at present impossible to estimate their relative importance as a cause of variation. Evidence has, however, been obtained both in Ceylon, and also especially in Java, that inherited differences in vigour, and in subsequent yield capacity, do occur and are of importance in determining the relative growth of different plants within a "jât."

The magnitude of the differences in growth observable in the field are considerable. Figures calculated from the data given in recent annual reports and from some recent experiments illustrate this. Even if we go so far as to assume that only a relatively small part of this variation is due to inherited causes, and is therefore to some extent under our control, such data would justify selection.

Bush type and yield interest us in the field — what of the factory? Data which Mr. Lamb has collected indicate that there are sufficient variations in the character of the teas made from individual bushes to justify further careful examination of the possibility of effecting

improvement by selection for desirable combinations of colour, pungency, quality, etc. This conclusion had also been reached in India and in Java.

Table showing Yields obtained from Best and Worst Bushes.

Experiment	No. of bushes		Relative Yield From 5%	10%	25%	50%
I.	3982*	Best	17.2	28.4	52.8	79.0
		Worst	2.1	4.2	6.8	21.0
II.	2530	Best	13.5	23.0	44.0	69.5
		Worst	2.2	4.3	12.3	30.5
III.	1000	Best	9.5	17.5	37.0	63.2
		Worst	2.5	5.0	15.0	36.8
IV.	1000	Best	8.5	16.5	37.5	64.7
		Worst	2.2	5.0	14.0	35.3
V.	1000	Best	7.5	15.0	35.0	61.0
		Worst	2.5	6.3	17.0	39.0
VI.	1515	Best	10.3	21.0	45.6	73.0
		Worst	1.2	3.5	10.4	27.0

* Including young bushes in bearing omitted in II.

Table showing Percentage of Yield given by the Worst Bushes.

Experiment	5%	10%	25%	50%	75%
I.*	2.1	4.2	6.8	21.0	47.2
II.	2.2	4.3	12.3	30.5	56.0
III.	2.5	5.0	15.0	36.8	63.0
IV.	2.2	5.0	14.0	35.3	62.5
V.	2.5	6.3	17.0	39.0	65.0
VI.	1.2	3.5	10.4	27.0	54.4

* Including 1,452 young bushes omitted in II.

There is thus no doubt that variations in bush type, in yield, and in the characteristics of the manufactured product, do occur from bush to bush, and that their magnitude is such as to justify selection within "jâts."

What then of that cherished conception of jât ? Frankly, gentlemen, as it is used in Ceylon it has little more meaning than terms such as mixture and group — certainly it has no such meaning as variety. Our cherished fields of "Assam indigenous" have had some connection with Assam in the past, but only too often that is about as far as one can go. This is not to deny that there is seed of *relatively* even type procurable in Ceylon — but nevertheless I have yet to see evenness in Ceylon comparable to that visible in the seed races of India and Java. And the India and Java seed producer is by no means satisfied or inclined to rest on his oars.

The possible methods by which more uniform and improved types may be sought are almost as variable in their different combinations as are tea bushes. Those of immediate interest are those by which the progeny of existing seed-bearers can be improved. Selection in the nursery, on plants twelve months or more old, can achieve a very great deal. No matter what improvements are made in seed-bearer type and uniformity, there will always be room for, in fact, need for, nursery selection; seed-bearers that produce entirely uniform progeny will not be produced in our lifetimes. To repeat what I said in another place — "even if we end up with clonal seed-bearers, there will still be some variation in the progeny, though very much less than at present, to justify the policy of taking the good and discarding the bad." I would ask you to refer to the last *Tea Quarterly** for a fuller discussion of nursery selection — it is too large a subject to be adequately dealt with here. It involves the selection of the young plants for type and vigour and there is definite evidence that such selection will result in larger average yields. To quote some Java results, published by Dr. Wellensiek (whom some of you have met), the overwhelming majority of plants in the nursery are relatively small, the number of plants decreasing very rapidly the higher the standard set. For instance, out of 19,817 seedlings, the size of the tops was below 100 grammes in 10,768 cases, between 300 grammes and 400 grammes in 595 cases, between 800 grammes and 900 grammes in 3 cases and between 1,300 grammes and 1,400 grammes in 1 case. In other words, we are busy planting up our tea areas with bushes, the majority of which grew relatively poorly in the nursery. As the result of extensive experiments, carried out on a very large scale, Dr. Wellensiek

* Vol, xi, page 8,

concluded that nursery selection before planting always resulted in improvement in the yield of the planted area, the increase being the greater the stricter the standard of selection.

It is, however, obviously desirable to reduce as far as possible, the variability of the seedlings by achieving greater uniformity of the bearers. Only selection by eye is feasible in large seed-bearers and that is quite laborious enough. The best method is to start with a careful bush-to-bush examination of, say, a hundred or more bearing bushes in a field planted with seed from the bearers. This will permit of a decision as to which type it is desired to select, and an estimate of the number and frequency of undesired types. If the bearer areas are then examined, preferably in early morning and late evening light, it will be possible to eliminate at least a proportion of the bearers which are making a large contribution to the heterogeneity of the progeny; this in spite of the fact that the foliage and growth characters of mature bearers tend to become superficially uniform, especially when they have for long been starved of nitrogen.

The value of seed-bearers lies in the value of the plants grown from their seed and the character of the progeny is the crucial test of a seed-bearer. It is therefore very desirable that, where possible, a small number of seedlings be grown from each bearer, thereby permitting the elimination of all those whose seedlings are wholly or largely of undesired type. This is a particularly suitable method where an estate has a relatively small number of bearers; also it has the great advantage that no assumptions about parent-progeny correlations are made. There is weakness in the method due to the occurrence of cross-pollination in tea but nevertheless the method is easily the best available for application to existing bearers.

There are possibilities, therefore, of making distinct improvement in our present position by working on existing bearers and their progeny. To what extent they are developed depends on you. If the industry asks for certain types that it likes, to go no further, then those types will be developed and the first steps in their production will be relatively quick. We have, after all, nearly every combination of bush characters possible around us and that at least gives us the opportunity of picking out those we want!

It is not, I would emphasise, the function of the Institute to tell you what jât of tea you want, nor where you can get it. Quite apart from the fact that the Institute must be like Caesar's wife, its function must be to get on with the more technical aspects of selection, elaborating methods and acquiring experience to aid the industry in its work of selection. To dive into the maelstrom of existing jâts would be relatively useless to the industry, more espe-

cially since the immediate measures to be taken are essentially matters of practical politics and demand no abstruse scientific theory or practice.

It is when we turn to the possibilities of effecting improvement by selection among bearing bushes that, at the present stage, research must pave the way for practical advances. Careful selection in the field of bushes of desirable type may be followed by selection of high-yielders from among them, and by the test of the latter for the quality of their produce. Each stage in such a selection will result in an enormous reduction in the number of bushes, and those that remain are correspondingly more valuable types. At each such stage a start may be made in establishing improved seed-bearers, whose relative efficiency will depend upon the stringency of the selection methods employed. The first steps in the field are thus very critical ones — the whole edifice is built upon them. A reasoned and carefully planned procedure, accompanied by the greatest care in carrying it out in the field, is essential.

The establishment of the seed-bearers may follow two courses: One method is to transplant the selected bushes and run them up as seed-bearers, selecting from among the relatively small number of progeny available from them those for establishing the new bearer area. The other is to multiply the selected bushes by vegetative propagation for "proof" and, subsequently, the establishment of bearers. I am not today going to talk about the special problems that arise in connection with clonal seed-bearers — we have quite enough to do at present to find the clones!

There remains the possibility of new introductions of wild tea types and of inter-varietal and inter-specific crosses. These are subjects that are more suitable for discussion as research problems than as matters of practical planting at the moment, but the exploration of their possibilities should not be neglected in any scheme of tea selection and improvement.

No one who is the possessor of seed-bearers which are the apples of his eye need be offended by anything I have said — but I hope he will take it to heart and consider it. In tea we cannot go on, as man finds in the case of all his crops, just cultivating the hedgerow plant and still be able to compete with those who improve their stock. At present, in tea as in other crops, Ceylon has allowed other countries to establish a substantial lead in the uniformity of available races and in selection among them.

I have attempted to show that selection is desirable and that it is feasible. In passing, references to the advantages selection may confer have been made, and I do not propose to dwell further upon

them. It is, I think, obvious that improvement in the material planted out in the field must result from the work of individual Superintendents on their own estates, and that so vast a problem cannot be left to a research staff so small in comparison to the work to be done, whose function should be to tackle the more specialised aspects of the problems associated with selection.

So I will have the temerity to propound a second question, to follow the first query of "to replant or not" — namely, are you content to plant the mixture of good and bad that Nature and man's improvidence provide us with today, or are you going to follow the lead of other countries and try to ensure that anything planted in the future is an improvement on what was planted in the past? The talk I gave in Nuwara Eliya might be taken as suggesting that the Industry in Ceylon should consider taking out a life assurance for the future: to continue the analogy, today's remarks suggest that if you do, it should be with a sound company.

DISCUSSION.

Mr. Huntley Wilkinson said that he had no doubt everybody would agree that Dr. Tubbs had done a great service to the Ceylon Tea Industry in bringing into prominence the serious question of the necessity for far more selection of both seed-bearers and young plants in his two recent lectures, the first given at Nuwara Eliya and the second given that day.

He thought it was high time they realised how uneconomic their methods have been in regard to their failure to reject both semi-undesirable seed-bearers and young plants in the nurseries from which they had for a number of years been supplying their old tea fields.

Regarding the very big question of a planned programme of replanting, he had lately taken out cycle yields on one estate of two very distinct fields which happened to lie side by side. A. was a field of 49 acres which could be described in loose phraseology as of very poor jât throughout. It was initially planted in an amazing manner with practically no lines showing anywhere. The former proprietor of the estate told him some 23 years ago that it must actually have been what was termed "Alavangoe planted."

B. was a very different proposition of 35 acres of excellent tea very well planted according to the standards of 50 years ago, even though the lines did run up and down hill. The jât of this field was perhaps as good as any in Dimbula and might be termed "Very even."

Since 1929 for three complete pruning cycles field A. had given a cycle yield of 1,349 lbs. per acre or an average annual yield of 539 lbs. per acre, as against 1,867 lbs. and 687 lbs, in the case of field B. The difference was 148 lbs. per acre.

He had calculated what the estate was now losing by having that area planted with bad jât rather than with tea such as was in existence in field B. It amounted to 7,252 lbs. of made tea per annum.

On the other hand replanting of that 49 acres at even Rs. 300 per acre would cost Rs. 14,700. To that sum should be added the annual loss while the field was giving no crop. That, computed at the old figure of 539 lbs. per acre for the 49 acres, worked out at 26,411 lbs. of made tea per annum, or a loss of Rs. 5,282.20 per year.

The Tea Control Department reckoned that at the elevation in question the young tea would not come into full bearing until its ninth year. Therefore, it would be fair to take four years as the length of time when that 49 acres would be entirely non-productive. The total loss for the four years would then be Rs. 21,128 plus the cost of planting which would amount in all to Rs. 35,828.80. If that was put against what they were now losing by leaving the field alone the difference would be found to be Rs. 30,027.20.

Dr. Tubbs might think his figure of spending Rs. 30,000 extra was based on far too short a period and that the expenditure should be spread over a period of perhaps 100 years. He wondered whether they would agree with Dr. Tubbs.

The 18th Report of the Imperial Economic Committee for 1931 states that between 1925 and 1928 in India alone as much as 80,000 acres were planted in tea. It was pointed out at the beginning of the first restriction period that the estimated quantity of tea available over and above what they would be producing under restriction would be in the region of 344 million pounds. The standard production at the moment was, he might add, in the region of 807 million pounds.

A Tea Broker in London remarked some five years ago that there would be 300 million pounds of tea over requirements if restriction did not come into force. Then commons would sell for 2½d. per pound and fines at 4d. per lb. In that case individual tea areas would probably not go entirely out of production but Distributing Firms would buy them up at their own prices and produce only what the consuming world would require.

That, of course, would mean that instead of the present number of planters looking after the Ceylon tea and rubber areas about one-quarter of that number would be required.

Mr. Wilkinson, in conclusion, said "If you think restriction will be continued for a period, make hay while the sun shines and do not spend a lot of extra money on replanting. If you think restriction is coming to an end after five years whatever you do will be futile."

Mr. C. K. Newton said that before joining in the discussion of that very interesting subject he wished to take the opportunity to congratulate Dr. Tubbs on the able way in which he had outlined such a knotty problem.

To his way of thinking if proprietors were content to follow the policy of "stay put" as expounded by Mr. Huntley Wilkinson then Ceylon would remain in the same position in which they were at present. Their competitors would go ahead and when the time came for the relaxation of any help by restriction schemes those holding shares in many Ceylon tea estates would find their scrip worth nothing.

Was it possible, he asked, that the big financial interests in Java and India would spend the money they were spending today on extensive replanting programmes if they thought anything of that "stay put" policy? Was it likely that those responsible for that progressive policy were entirely lacking in foresight and business acumen?

Possibly the main reason for Ceylon being behind other tea-producing countries in the production of purer seed and in replanting in general, was the relatively small unit here as compared with their neighbours. That applied especially to up-country districts where small proprietary estates had been turned into companies, sometimes controlled by Boards who wished to obtain the highest possible immediate dividends with little thought of the future.

A large unit, of which there were a minority only in the Island, could spend money on replanting areas yearly without affecting the dividend to any great extent, whereas the small one could not as the dividends would fall, much to the annoyance of shareholders.

That was all the more reason for giving as much publicity as possible to the problem in an endeavour to make shareholders and Boards of Directors realise the necessity of spending money for the purpose of replanting to safeguard the future of the interests concerned.

The question of quality referred to by Dr. Tubbs was to his mind a most interesting one when replanting was considered. He hoped the Institute would be able to tell them more on that subject before long.

If they could be assured of improved quality leaf as well as higher yields, then the economic aspect for recommendation to Boards of Directors became far more acceptable than higher yields alone. What might not appear to be sound financially from a yield point alone, taking into consideration the loss of revenue when the new areas were growing, might become so when linked with quality.

He felt that the problem had been in the minds of most planters for years, but that lack of publicity had been one cause of apathy on the part of those who controlled the purse strings.

His first experience of replanting tea was in 1920 on a block of some five acres only. That would show that the problem had received consideration, not just recently only.

He wished to ask Dr. Tubbs about how many plants he would expect to get from a maund of tea seed, after the careful manner of selection indicated.

Dr. Tubbs said that as far as he personally was concerned he agreed much more with Mr. Newton than with Mr. Huntley Wilkinson. Mr. Huntley Wilkinson drew a very gloomy picture indeed. If things were going to be as bad as all that it would be better for them to shut up shop and go home now! Mr. Huntley Wilkinson's arguments were based on policy and it was not his business to decide upon matters of policy but to draw their attention to the principles to be considered in making the decision. But since the subject of replanting had come up, one or two comments were perhaps desirable. It must be remembered that he had not said that one should replant everything, or that one should rush into replanting 50 acres at a time on a small estate as Mr. Huntley Wilkinson envisaged. They should not lose sight of the fact that with the increasing acreage in the world today they would have to exercise discrimination as to what areas should be replanted. Some areas would never be worth replanting and there was no point in throwing good money after bad.

In the first place Mr. Huntley Wilkinson's figure of 100 years went far beyond what he personally would suggest. He could not look forward to 100 years! But when considering the question of replanting they should take into account the capital and revenue value of the estate for at least 10 years.

Mr. Huntley Wilkinson apparently would prefer to indulge in replanting cheaply while continuing to produce the maximum crop for which he could get extra coupons at a reasonable price. For that matter everyone would like to do that, but, as far as he knew, they had little chance of doing it. If one were content to utilize the opportunity afforded by restriction and then commence replanting, the future had hope, and the present had reduced profit. One could not have it both ways.

Mr. Huntley Wilkinson's conclusion summed up his whole position — the wish to produce the full crop and to wait until someone else had discovered exactly what profits and benefits accrued from replanting. That meant that for the sake of present profit he was willing to envisage a future with a low actual and potential yield, which might one day even fall away to nothing. The alternative was to replant with material which would give a higher actual and potential yield and thereby safeguard the future to the best of one's ability. Were we to learn nothing from cinchona and rubber?

It should be remembered that Mr. Huntley Wilkinson's financial calculations were based upon comparison of a poor jât field with an old good jât field — good probably only by the standards of fifty years ago.

What he was suggesting was that if they were going to replant it must be with improved material and that they should take measures to see that it *was* improved material. In other words he was striking at the very root of Mr. Huntley Wilkinson's argument.

A time might come when they would be forced to obtain what yield they could to reduce their cost of production per pound, and if that time ever did arrive Mr. Huntley Wilkinson would pin his faith much more on his higher or good jât areas than on his old poor jât areas.

The actual decision as to whether replanting should be undertaken or not must be the responsibility of the management of each individual estate, but whatever decision was arrived at, he felt it was of the utmost importance that the subject be carefully considered and that they cease to regard every tea plant as equally good, and exercise more discrimination in their selection of them.

As regards Mr. Newton's question it was not possible to say with any exactitude how many selected plants could be got from a maund of seed. It would depend both on the seed and on the standard of selection set. As was to be expected, the stricter the standard of selection, the better but fewer the final plants. As a rough guide in the present state of affairs he would suggest about 4 to 6 seeds per plant required.

With regard to quality, they must not run before they could walk, and while a close eye would be kept upon the possibilities of achieving improvement in the general average of quality, it would be wrong to hold out any immediate prospect of such advances. That, he considered, was in no way an objection to what he had said that day. Selection provided the opportunity of improvement. If they selected now for characters they could estimate, it would facilitate later selection for quality. The fact that the sun was desired was no reason for refusing the earth in the meantime.

He could assure them that the programmes being worked out by the Institute quite definitely included selection in regard to the quality of the manufactured product. It would be quite wrong for him, however, to hold out any hope of being able in the immediate future to make such a substantial contribution as would enable Superintendents to pick out those bushes which would give the more desirable manufactured product.

He did not see any probability or possibility of Ceylon adopting such a strict standard of selection as was likely materially to increase or decrease the average of the quality of the tea derivable from the plants put out *at present*. That must be a matter for much more detailed research and experience.

Mr. L. B. Green said that he wished to support most heartily the remarks made by Mr. Newton. He thought a forward policy would always be better than a "stay put" policy. Supposing they adopted a forward policy, the question he wished to ask Dr. Tubbs was that when certain qualities in the finally selected plants, such as vigour of growth, large leaf, long hairy tips, high yield, least possible tendency to banji, general hardihood and resistance to disease, were desired, how could they make their selection out of a nursery full of plants when it was possible to judge by eye only in regard to the first three characters?

Dr. Tubbs said that he was glad he had prior notice of that question which enabled him to be more detailed in his answer. As Mr. Green had stated, vigour of growth, size of leaf, and hairiness could be judged to a greater or less extent in the nursery according to the individual attention given to each plant. The further characteristics he mentioned could not be *directly* estimated, but with the exception of quality, all tended to fall under the classification of vigour.

Dr. Wellensiek had shown that vigorous nursery plants yielded more highly than the less vigorous; again high yield was likely to be associated with reduced frequency of banji. They might expect, therefore, the most vigorous nursery plants to be on the average high yielders and to form less banji than more weakly plants. General hardihood and resistance to disease would to a greater or less extent also be covered by the selection for vigour, owing to the effect of those characters on the growth made by the plant in the nursery. Little more could be safely said at that stage on those subjects, and when it came to that of quality he preferred to say nothing at all save that he did not envisage Ceylon carrying nursery selection to a sufficient extreme to influence quality yet awhile. The positive side of the question — how to select for quality in the nursery — could not be answered at present and he could hold out no immediate hopes of answering it.

Mr. Green asked if it were possible to connect the shape of the leaves with some or any of the above qualities and if so, which?"

Dr. Tubbs in reply said it was at present impossible to say that any leaf character was definitely associated with any particular characteristics of growth or composition. While it was not impossible that some such relationship might one day be detected, his opinion was that the chance of this would remain very small indeed until much more uniform races than we had in Ceylon today were obtained. In other words, while it might be discovered one day that a given leaf character is linked with a given character of the manufactured product, such a linkage is less likely to hold true over the wide range of types now available than within a relatively very uniform race. Such tests would therefore await the attainment of that uniformity within jâts which he had suggested it was desirable to work for.

Mr. Green asked if the venation or serration of the leaves was an indication of any particular type.

Dr. Tubbs replied that almost any leaf or bush character was of assistance in achieving uniformity, provided the plants were grown under similar conditions, and provided that for accurate examination comparable leaves were examined, say the fifth leaf or dormant (banji) shoot. The best way of comparing leaf shape, etc., was to lay one leaf upon the other, but the examination of venation did not permit of this. Difficulty might be found in deciding what was a primary vein for counting purposes, and he did not attach much value to this test at present.

Mr. Green then enquired whether, having regard to the qualities desirable in a seedling, Dr. Tubbs could say whether dark leaf types or light leaf types were the better?

Dr. Tubbs said that while it appeared probable that it would be possible to select as desirable types from one as from the other, in general there was little water-tight data as to the relative merits of light and dark leaf types. In Ceylon real light leaf was fairly rare and tended to occur as a small percentage among predominantly medium or very dark leaf types. In India the impression seemed to be that light leaf forms might be desirable for quality but that some light leaf jâts were delicate. In Java Dr. Wellensiek concluded that light leaf forms were more affected by external conditions than dark leaf forms. From the data published by the latter worker he had calculated the following figures. These represented the relative growth in height of four different jâts at varying elevations.

Elevation		Jât 1	Jât 2	Jât 3	Jât 4
5,900 feet	...	82	103	104	104
3,800 ,,	...	93	95	100	100
2,700 ,,	...	97	100	101	94
2,600 ,,	...	107	95	100	104
1,000 ,,	...	108	97	98	101
500 ,,	...	112	111	97	97

Jâts 1 and 2 were light leaf forms — jât 1 was known as a rather delicate type and grew better the lower the elevation. Jât 2 seemed much more adjustable to local conditions. Jâts 3 and 4 (dark leaf types) did not exhibit so great a range of variation and seemed to do about the same at all elevations. Jâts 1 and 2 shewed that one could not say that all light leaf forms were equally delicate and they could not therefore be judged as a class. The idea that dark leaf forms were less delicate, while possibly not universally true, did appear to be roughly correct. As an example he could quote some 6-year-old unpruned plants that he wished to transplant as seed-bearers. They were moved to the new situation with the greatest care, but the two light leaf forms died back to near the collar, while the two dark leaf forms continued to flower and did not drop more than one to two leaves.

Mr. Sydney Smith said that 20 years ago in Ceylon it was found that it was from something like 30 per cent of the trees that 70 per cent of the yield was obtained and 70 per cent of the trees contributed very little.

What he wished to know was whether it was better to replant or adopt what the Tea Research Institute called rejuvenation. Supplying was a very successful business now and so a lot of the tea could be rejuvenated. He wished

to know what the objection to that was. They were definitely going to make very much vaster progress. Why had Dr. Tubbs omitted that question? What was nursery selection going to cost? There seemed to be no short cut as there was in the case of rubber — budgrafting.

Dr. Tubbs said he would start at the end first because he did not wish to miss referring to the subject of the cost of selection. So far as he had been able to go into the question of the cost of nursery selection it would appear probably a quite feasible, economic proposition provided they were growing their own plants. The possibility should not be lost sight of of an individual estate getting at least some bearers of its own that it really knew something about. This would make a material contribution towards reducing the cost of putting out thoroughly selected plants.

He was not suggesting that they should rush into selection. In fact he had been careful to lay emphasis on the necessity of very carefully considering each step in the process. Just as he thought the greatest possible harm would be done to the industry in Ceylon if they neglected to consider the question, nearly as great harm could be done by rushing into the thing without thinking about it and thereby getting the whole process into bad repute.

At the present price of tea seed it was feasible to select with advantage provided they grew their own plants. It was impossible to go into figures like that and give any general ruling as to whether it paid to discard 50 per cent of the plants or not. That depended on the cost of producing those plants, getting seed, etc. Dr. Wellensiek had shown increases in yield as a result of nursery selection of up to 30 per cent.

He had not referred to rejuvenation largely because the subject of replanting and selection were in themselves far too big to allow the introduction of any other question. If they had a good area planted closely enough to ensure their being able to keep the soil at least in its present state, and the tea was all good type and good potential yielders, then rejuvenation must be seriously considered. But if they were not in that happy position replanting must appear more advantageous. The great point about rejuvenation was that they got the bushes back into yield quickly and relatively cheaply.

Mr. Sydney Smith enquired why Dr. Tubbs advised them to close up holes as soon as they were ready, whereas they were told in the past to leave them open for some time.

Dr. Eden said he could not imagine why Mr. Sydney Smith was advised to do that. He agreed entirely with Dr. Tubbs that when a hole had been excavated and the situation shown to be one in which a tea bush could be put in, there seemed no reason at all why it should not be filled the same day or the next. Once the soil was broken up so as to enable the water to percolate freely that was all that was necessary to secure root expansion and aeration of the soil,

The Chairman inquired whether there was any reason why they in Ceylon should not import their tea seeds from the more progressive countries.

Dr. Norris said at present that was not possible owing to the terms of the Tea Restriction Ordinance which limited any exports of tea seed or material to small quantities required by scientific institutes for technical purposes. No provision had been made for export of seed or other material on a commercial scale. So far as Indian seed was concerned there was a complete ban on the import of this into Ceylon under the Plant Pest Ordinance. That was likely to be modified in regard to seed required by the Institute for its selection experiments.

He thought, moreover, it should not be assumed that jâts which had proved the most suitable under Indian conditions would necessarily be those best adapted to Ceylon. Their problem was not likely to be solved quite so easily as that.

Dr. Tubbs said that he was not suggesting that they had no types in Ceylon suitable for their requirements, but he felt it necessary to emphasise the tremendous lead in establishing desirable uniformity that other countries had gained.

The Chairman thanked Dr. Tubbs for having given such an interesting address and also for providing them with an opportunity for free discussion. He also thanked all those who participated in the discussion.

THE USES OF STATISTICS IN FIELD EXPERIMENTS WITH RUBBER TREES.*

There is an old gibe that there are three grades of falsehood:— "Lies, Damned Lies and Statistics." The intention here is to show that statistics, properly used, so far from being the acme of falsehood, is indeed common-sense in a highly developed degree. Huxley defined Science as "organised common-sense," and this is a definition which is specially appropriate here, for the application of statistics to field experiments has made it possible to carry these out in a scientific manner not before possible, and to interpret their results with a high degree of scientific precision. Statistics have in fact brought a large amount of common-sense to bear on the conduct and interpretation of field experiments.

It must of course be understood that the term statistics is used here not in the loose sense of the mere collection and classification of numerical data, but in the specialised sense of the use of higher mathematics for the analysis and interpretation of data, and for the design of experiments. It is not intended to attempt to explain the mathematical principles involved or the manner of their use. That is the job of the higher mathematician himself — so much so that although other scientists have to understand the general principles of the application of statistics to the design and interpretation of experiments, it is nowadays best left to the specialized mathematical statistician to undertake the actual work of applying his methods. It is for that reason that the Rubber Research Institute has just appointed a Statistician to its staff.

WHAT IS AN EXPERIMENT?

An experiment is defined in Webster as "A trial . . . . . made to confirm or disprove something doubtful" — and in the case of rubber-growing, the "something doubtful" is whether a certain method of treatment or a certain type of material is better than others, and therefore worth while to adopt.

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* The substance of a lecture given at the Annual Conference of the Incorporated Society of Planters at Kuala Lumpur, 1937, by Mr. H. J. Page, Director of the Rubber Research Institute of Malaya.

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Anyone can carry out an experiment, and can obtain results therefrom. But that is less than half the battle. The experiment may have been so badly designed or so badly conducted that it cannot possibly give results capable of valid interpretation; or it may have been well designed and executed, and yet the meaning of the results may be obscure or misunderstood.

Let us take as our first example an experiment intended to compare only two things, — or "treatments" as they are usually termed, — for instance to compare the yielding power of one clone with that of another, or with that of seedlings, or to ascertain the relative merits of two tapping systems, or the effect of a given fertiliser on the yield of rubber trees. What would appear at first sight to be the obvious way of doing this would be to take two adjacent plots of land as alike as possible, and to apply the two treatments one to each, and then to compare the results obtained.

Now the crux of the whole matter is that more often than not this is an entirely misleading and unreliable way of doing a field experiment, and the chief object of this paper is to explain why this is so.

"SINGLE-PLOT" EXPERIMENTS.

The practical man is strengthened in his unwillingness to accept this verdict by the fact that the earliest and best known agricultural field experments were carried out in this way, as "single-plot" experiments: that is, with only one plot for each treatment. Many of the most important advances in scientific agricultural practice are based on the results of such "single-plot" experiments. The world-famous manuring experiment on Broadbalk Field at Rothamsted for instance, from the results of which our knowledge of the main principles of manuring was first derived, is a "single-plot" experiment, started in 1843 and is still going on.

The reason for this is that if the effects which the experiment is intended to study are big enough, "single-plot" experiments will establish them with sufficient certainty. The Broadbalk experiments just referred to showed increases in yield of the order of 50 or 100 per cent or more — in fact at the present time the plot receiving no manure gives barely 10 bushels, whilst the plot getting a liberal "complete" manuring gives over 30 bushels: an increase of about 200 per cent over the control! If we knew beforehand that the effects to be studied in our field experiments were always as big as that, we could probably do well enough with "single-plot" experiments, if indeed we should then bother to do any experiments at all! But unfortunately the effects we wish to measure nowadays are more often only a matter of differences of 10 or 20 per cent instead of 100

or 200 per cent. They may be no less important for all that. A 20 per cent increase in yield of rubber, yielding before treatment 400 lb. per acre, *i.e.* an increase of 80 lb. per acre, may represent a handsome, profit, perhaps 100 per cent on the cost of getting it. But an increase of 20 per cent usually cannot be detected with certainty from a "single-plot" experiment.

Two adjacent tasks, one of which is manured and the other not, may give yields of 400 and 480 lb. rubber respectively, and yet the difference may have nothing whatever to do with the manuring. Even if previous records for the two tasks indicated that they then both gave 400 lb., the difference of 80 lb. after manuring may have been due to other factors entirely unconnected with the manuring. Moreover, unless special steps have been taken to ensure that the previous records do actually represent the true yields of the two tasks, there is no guarantee that they do; for tappers are notoriously prone to level up their deliveries of latex from adjoining tasks. All the same, as explained later, the existence of previous records does help to improve the reliability and to simplify the layout of experiments on mature rubber. But for "single-plot" experiments for which suitable past yield-records are not available, such as comparisons of different planting material; or cultivation and manuring experiments on young rubber; one is entirely at the mercy of the variations in the fertility of the soil over the area chosen for the experiment, as well as many other varying factors outside one's control.

PLOT VARIABILITY: "UNIFORMITY" TRIALS.

Before going any further to explain this, a few examples follow:

Nos. I-V show the yields from the untreated control plots from various experiments.

Nos. VI-VIII show the yields from adjoining plots, or "uniformity trials."

I

			973
	926		
		807	
944			

Difference between adjacent plots in centre = 14 per cent.

II

		511
680		
	531	

Difference between adjacent plots = 25 per cent.

III

		202
211		
	206	

Exceptional uniformity.

IV

119		120					162

Right hand plot 35 per cent in excess of other two.

V

179	179			155	153			106	121

Good agreement of adjacent plots in two cases, but 14 per cent difference in the third case, and a pronounced "drift" from high yields on left to low yields on right.

VI

102	93	90	92	97	98	91	89	99	100

A "uniformity trial" showing fair agreement, but a range of 13 per cent between highest and lowest yields.

VII

A	B	C	D	E	F	G	H
3.5	3.5	4.3	3.5	3.3	3.3	3.9	4.3

Yields from adjacent tasks (in gms per 100 trees per tapping) Differences between adjoining tasks, and the same as percentages, are shown below.

Nil	0.8	0.8	0.2	Nil	0.6	0.4
—	20	20	5	—	15	10%

VIII

A uniformity trial of 78 plots. The yields showed a total range of variation of nearly 60 per cent, and examples of yields from adjacent plots are as follows:—

73, 133	Difference = 58%	of mean	
103, 84	,,	,, 18%	,,
84, 112	,,	,, 28%	,,
89, 117	,,	,, 27%	,,
106, 79	,,	,, 29%	,,
115, 83	,,	,, 32%	,,

These are all actual instances of the yields obtained from adjoining or closely situated tasks of rubber, to which no different experimental treatments have been applied, *i.e.*, which have been treated, as nearly as practicable, exactly alike. Admittedly some of these are chosen deliberately as horrid examples, but they are none the less actual instances of the variation that may occur. Others of the examples displayed show that sometimes there may be a high degree of uniformity. The point is that one never knows with even reasonable certainty what the extent of this natural variation is. All the areas in question were chosen to represent the most uniform land and rubber available on the estate. This is an effectual answer to the planter who insists on using only single plots; and who says: "The other chap may have to use an elaborate modern experiment to get results, but I know my soil and my rubber so well that I can rely on the results of single plots." That is as often as not just sheer nonsense. Agricultural scientists have found by bitter experience that even on the most level and uniform-looking land, with apparently uniform planting material, be it an annual crop or perennial, differences in yield of the order of those shown on the diagrams are only too commonly found. Orginally agricultural scientists were just as unwilling and reluctant as the farmer or planter, to adopt the modern type of elaborate layout of field experiments. The remark of a certain distinguished professor of agriculture: "Damn your duplicate plot; give me single plots and I know where I am," is typical of the attitude which has had to be given up after hard experience. He thought he knew where he was, but more often than not he would have been quite wrong.

The reason for this uncertainty is that with all natural material, living or inanimate, there is a greater or less degree of variability, which is not the case with pure substances. The chemist working with pure substances is largely free from this trouble. He does not have to do an experiment, for instance, to find out whether sulphuric acid and caustic soda will react together and neutralise one another. He knows that they always will; that 98 grams of H_2GS_4 will always be exactly neutralised by 80 grams of NaOH to give 142 grams of Na_2SO_4 ; and that a fixed and constant amount of heat will be liberated. All molecules of sulphuric acid or of caustic soda are chemically alike. But no two individuals of any living kind are exactly the same, nor are any two samples of natural material, and the behaviour of an individual or a sample cannot be predicted with certainty. It is this innate variability of natural material, and of the environmental conditions, which places experimentation with such materials, and particularly with living materials, on an entirely different plane from experimentation with pure substances. The statistician's job is to study variability and its effects, and it is for that reason that the assistance of statistics is so necessary in the design and interpretation of field experiments.

FACTORS RESPONSIBLE FOR VARIATION.

Let us examine the factors responsible for variation in field experiments with rubber trees. In the first place there is the variation in "fertility" between different plots of land. In a properly designed experiment this is the biggest source of variation. Even the most uniform-looking area of land may possess quite big variations from one place to another. Even if the surface soil is quite flat and apparently uniform, there may be invisible differences in the nature and depth of the subsoil. The sporadic occurrence of a hard pan, at varying depths, is a special case of this, and instances often occur in which the existence of such a hard pan is first discovered only as a result of poor sickly growth of trees in patches. Moreover, an apparently uniform top soil, of uniform depth, may yet hide differences in fertility through differences in the previous croping history of the area; a part may have been heavily depleted of available plant food by previous cropping with, say, tapioca. On many estates it is practically impossible to find any level areas of land, and experimental areas have to be on hillsides, with considerable differences in the soil, and in conditions such as drainage, between the higher and the lower land.

Added to these inherent variations in the soil, are variations in the rubber trees. By choosing plots of the size of a task, with over 200 trees, the effect of the large tree-to-tree variability, which occurs especially with seedling rubber, is largely eliminated; and differences

in age or origin of trees should always be avoided by choosing an area which was all planted at the same time with uniform material (except of course in an experiment for the comparison of different sorts of planting material). There still remain the effects of uneven attack by pests and diseases, of differences in tapping history or in average height of tapping, whilst there is also the possibility of lack of uniformity in the conduct of the experiment and in the collection of the results.

SIZE OF UNCONTROLLABLE VARIATION.

The uncontrollable variation due to differences in soil and subsoil, — or "soil heterogeneity" as it is usually called, — will sometimes be slight and sometimes be large, whilst the other factors instanced above will similarly differ in their incidence from place to place and time to time. Thus the sum-total of the effects of all these factors, as reflected in the growth or yield data obtained from the plots, will also vary between wide limits, as shown by the examples above, where in some cases the yields show a very satisfactory approach to uniformity, as between different plots, whilst in other cases the differences may be over 25 per cent. The important point is that one can never be sure beforehand what the position is.

Suppose that the two plots A and B of example VII above had been chosen to compare two clones, and that a difference of 20 per cent had shown up between them in growth, or in yield when they came to tapping, then one would in fact be reasonably safe in drawing the conclusion that the two clones did actually differ, on that soil, by about 20 per cent in their growth or yield. On the other hand, suppose that plots C and D had been chosen, without the knowledge which the "uniformity" test has here revealed. The two clones might be identical in their inherent yielding capacity, and yet the natural "soil heterogeneity" between the two plots would result in a difference of 20 per cent, which would be quite erroneously interpreted as a difference in the clones themselves, if the results of only that one single-plot experiment were accepted as valid.

This is sufficient to demonstrate how unsafe it is to rely on the results of "single-plot" experiments — save under certain special circumstances mentioned later.

REGULAR AND IRREGULAR VARIATION.

Uniformity trials such as those which are represented above, are trials in which the area in question is divided up into plots which are harvested separately, without any difference in treatment. These enable *fertility contour diagrams* to be drawn. These

are similar to the height contours so familiar on ordinary maps. There are peaks of high fertility, hollows and valleys of low fertility, and intermediate slopes of average fertility. Now if changes of fertility of this sort occur *within* one experimental area, it is clear that the variation from plot to plot is irregular, though contiguous plots are likely to be less different than those more widely separated.

On the other hand the scale of the fertility contour map may be smaller, so that the whole of an experimental area falls on a patch of regularly varying fertility. The pitfalls into which a regular fertility gradient or "drift" of this kind can lead, are well illustrated by a case recorded some years ago, which is illustrated below.

0	10	20	30	40	50		Cwt. of lime per acre
A	B'	C	D	E	F	X	
20	22	24	26	28	30		Yield.

There were seven plots in a row, the first receiving no treatment, and five others receiving, in order, 10, 20, 30, 40 and 50 cwt. of lime per acre. The yields obtained may be represented by the the figures underneath the plots, *e.g.*, a steady increase in yield from 20 to 22, 24, 26, 28 and 30 units — apparently a clear case of increasing benefit to the yield from increasing amounts of lime. Fortunately, (or unfortunately!) a second control plot was included at the other end, marked X on the diagram. The yield from that control plot was 32! The lime had really no effect on yield at all; all that the experiment showed was a uniform fertility drift increasing from left to right. But for the after-thought of adding a duplicate control plot at the other end, this would not have been discovered, and it would have been concluded that lime had an increasingly beneficial effect on that soil, in dressings at least up to 50 cwt. per acre!

It might have happened, however, that the experiment was laid out on land with a peak of high fertility in the middle, in which case the two control plots one at each end would have given similar yields, and it would have been concluded that the land was of uniform fertility, unless another control plot had been included in the middle to reveal a yield much higher than those from the plots at the two ends.

One of the earliest methods of guarding against misleading results of this kind was to make every other plot a control plot, as below:—

A	B	A	C	A	D	A	E	A	F	A

A = Control plots. B, C, D, etc. = Treated plots.

Then by comparing the yield of each treated plot with the average of the yields of the two control plots one on each side, a more accurate measure of the treatment was obtained. But what a wasteful and cumbrous method of experimenting! Half the total number of plots is given up untreated to controls, and there is still only one single plot for each treatment. All the same, this was a definite move in the right direction, for it introduced the principle of repetition, or "replication" as it is now called, (as a useful word to cover duplication, triplication, and so on, up to any desired degree of repetition).

REPLICATION.

In view of the chances of variation between plots, it is obvious that if there are *several* of each treatment, distributed over the whole area, the *average* result obtained from all the replicate plots of any one treatment is much more likely to represent the real yield obtainable with that treatment under the average conditions of that area, and comparison between such averages for different treatments is far more likely to be a true indication of the relative effects of those treatments.

Replication is the first essential of a reliable field experiment, and in all modern experiments each treatment (including the control, if any) is replicated the same number of times. Let us take four-fold replication, (that is, quadruplication), as an example. In the following diagram is an experiment with four treatments, A, B, C and D, with four plots for each treatment.

A	B	C	D
D	A	B	C
C	D	A	B
B	C	D	A

This is typical of the kind of experiment first adopted after the need for replication came to be recognised. It includes adequate replication, but its arrangement is *systematic*. A, B, C and D always follow one another in the same order.

EXPERIMENTAL ERROR.

Now we have seen that owing to "soil heterogeneity" and other uncontrollable factors, the results obtained from an experiment are lacking in accuracy to a greater or less degree. The extent to which

the results are therefore not completely accurate is the experimental error, termed mathematically the "standard error" or the "probable error" according to the mathematical convention used to calculate it. It is expressed as ± X, where X is a number; the higher X is, the more inaccurate the experiment is and the less reliable are the results. A probable error of say ± 10 per cent applying to an average yield of say 100 units from a given treatment, means that owing to the influence of uncontrolled variation in the conditions of the experiment, that average is inaccurate, and that there is a good chance that the true value might be only 90 on the one hand, or 110 on the other, or anywhere between; it is impossible to say, between those limits just what it really should be.

The object of a properly-designed field experiment, so far as the experimental error is concerned, is not only to make that error as low as possible, but also, *equally important*, to make it possible to calculate that error accurately. The use to which the results of an experiment are put is to decide how the effects of different treatments compare. This involves studying the *differences* between the average results for the two treatments under comparison. Supposing one treatment gives an average of 100 and another an average of 120. If the "probable error" of each is ± 10 per cent, that means that the treatment with the average of 100 may be taken as being possibly anything from 90-110, and the other as anything from 108-132. The difference between the averages for the treatments may therefore be anything from −2 to +42! The apparent superiority of 20 per cent of the second treatment over the first has changed into the complete uncertainty, whether perhaps it is no better, or whether perhaps it is very much better. This is taking rather an extreme and crude example — the mathematician has more elegant and exact methods of treatment — but it serves to illustrate my point. Supposing the probable error has been wrongly estimated and that it is really only about 5 per cent instead of 10 per cent, what a difference that would make to the value of the results.

The fault of the systematic arrangement of plots with replication, is that whilst it reduces the experimental error, it does not allow the true value of that error to be calculated, and as just explained, it is not much use improving the accuracy of results if you do not know what that accuracy is.

RANDOMISATION.

The other requirement of a properly laid out experiment, in addition to replication, is "*randomisation.*" That simply means that the arrangement of the plots is not systematic, but at random, —

according to chance. If the previous diagram is compared with the following one

A	D	B	C
D	C	A	B
C	B	D	A
B	A	C	D

it is seen that whilst in the first one, A, B, C and D always come in the same order in each row or column, in this one that is not so, — they are arranged at random in each. The order in which the treatments are assigned, in a randomised arrangement, is one of the many in which they might come if dealt from a shuffled pack of cards, or thrown with dice, or drawn from a hat.

It is a requirement of the mathematical statistics whereby the standard error is worked out, that if the estimate of that error is to be *valid*, the arrangement of treatments in each experiment, or section thereof, must be at random, not systematic. We have seen already that it is not any use calculating the experimental error unless it is really an indication of the true error, and here it must be taken on trust that randomisation is essential for that purpose. The mathematical arguments underlying that fact are perfectly sound but far too involved to explain now.

RANDOMISED BLOCKS AND LATIN SQUARES

The commonest and best-known arrangements in which the great majority of properly-laid-out field experiments are included, are *Randomised Blocks* and *Latin Squares*. Many will be familiar with these from the schemes of experiments they are doing in co-operation with the Rubber Research Institute, the Department of Agriculture or with other concerns. It is hoped that after the attempt to explain why they are necessary, and the further attempt later on to explain how they work, planters will be happier in their minds about what must often have seemed the rather unreasonable requirements and conditions they are asked to conform to in such experiments.

In the case of the *Randomised Block* experiments illustrated below.

Two Examples of Randomised Block Experiments

(Plot yields shown only for Control (A) Plots)

							Block Totals
Block	1	A 17.5	C	D	B	E	102
,,	2	E	C	B	D	A 27.2	122
,,	3	E	B	D	A 18.7	C	127
,,	4	A 21.8	B	D	C	E	128
,,	5	D	C	E	A 23.6	B	134

Standard Error of Difference between Treatment Averages = 8.8%
Significant Difference between Treatment Averages = 17.6%

An example of a Randomised Block experiment showing rather a high experimental error, as was to be expected from the big variation in yields from Control (A) plots.

							Block totals
Block	1	D	C	E	A 3.2	B	16.6
,,	2	E	C	D	B	A 3.2	16.9
,,	3	C	A 3.2	D	B	E	17,3
,,	4	A 3.1	D	B	C	E	16.7
,,	5	E	B	D	C	A 3.1	16.9

Standard Error of Difference between Treatment Averages = 2.6%
Significant Difference between Treatment Averages = 5.2%

An example of Randomised Block experiment with a low experimental error, as was to be expected from the uniformity in yields from the Control (A) plots, and in the Block totals.

We have five treatments, A, B, C, D, E, and fivefold replication, so that there are five separate blocks. In each block there are five plots, one for each treatment, and the allocation of these treatments is entirely at random, that is, in the order in which they might be drawn from a hat. The random arrangement is different in each block, representing a fresh drawing from the hat for each.

An Example of a Latin Square Experiment

R. R. I. Experiment Station Block 6, 1936

Letters = Treatments

Numbers = Plot yields

						Row Totals
B 391	A 330	C 374	D 362	F 339	E 372	2168
C 414	D 399	F 384	E 387	B 392	A 375	2351
F 395	C 445	E 336	A 391	D 362	B 329	2258
D 393	B 394	A 383	C 440	E 361	F 386	2357
A 378	E 373	B 391	F 399	C 390	D 373	2304
E 337	F 372	D 350	B 397	A 352	C 454	2262
Column totals 2308	2313	2218	2376	2196	2289	2283

Treatment Averages	A ...	368	97%
,, ,,	B ...	382	100%
,, ,,	C ...	420	110%
,, ,,	D ...	373	98%
,, ,,	E ...	361	95%
,, ,,	F ...	379	100%

Standard error of Difference between Treatment Averages ... 3.44%

Significant Difference ... 7.0 %

The *Latin Square* is a slightly more restricted arrangement, in which the number of replications is the same as the number of treatments. We have such a 6×6 Latin Square illustrated above.

It is not necessarily a geometrical square, for the individual plots may be longer than they are broad, but it is called a square because it has the same number of plots, 6, in each direction. It may be regarded as six randomised blocks, either vertically or horizontally, and it is so in fact, in so far that the arrangement of the treatments in each row or each column is at random, but with this important restriction, that any one treatment must not occur more than once in any one row or in any one column.

An essential feature of experiments of this type is that the *total* yield for a block (in a Randomised Block experiment) or from a row or column (in a Latin Square experiment) is contributed to by each treatment alike. In the above Latin Square, one-sixth of the area of each row or column is untreated control (A), one-sixth has received treatment B, one-sixth treatment C, and so on; so that all treatments have an equal share in their effect on the block, row or column total. The comparison of these totals thus does not involve main differential treatment effects, and is an indication of the variation due to other uncontrolled factors. Variations in "soil heterogeneity" can be observed from variations in block, row or column total, and by a mathematical treatment of those totals, a large part of the error caused by "soil heterogeneity" and the like can be calculated and can be eliminated from the error attaching to the comparison of the treatment totals, so increasing the accuracy of the experiment.

"SIGNIFICANT DIFFERENCE."

An attempt will now be made to explain what is meant by another term which is used very frequently in stating and interpreting the results of an experiment, the term "Significant Difference." To do this, it must first be remembered that the irregularities due to "soil heterogeneity" and other causes are distributed more or less at random over the experimental area, and that it is a matter of *chance* whether any particular plot will be on an area which is above or below, and much or little above or below, the average. The mathematical treatment of the results must therefore be one which deals with the assessment of *chances* or *odds*, or as the mathematician calls it, *probability*.

When a "standard error" is said to be $\pm$ 10 per cent that does not mean that the result could not possibly be even less accurate than that, but that the odds are in favour of the inaccuracy not exceeding $\pm$ 10 per cent. Since there are so many variable factors

not under complete control in any experiment: soil, weather, disease, Tamil coolies, etc., and since the incidence of these factors can never be exactly known, there *must* always be *some* uncertainty regarding the accuracy of the results obtained, however well the experiment has been designed and carried out. The object of the application of statistics to the design and to the interpretation of field experiments is to reduce that uncertainty as far as possible, and to define it. This is done by stating the experimental error and the "significant difference" in terms of probability or odds. It is open to anybody to decide what degree of probability he is willing to accept as good enough for him in practice, and circumstances may often dictate whether long odds or short odds are worth while.

The odds that are usually accepted by the scientist in the interpretation of field experiments are 20 to 1, or 95 per cent probability. (P=0.05). As explained already, there is always a chance that the difference between the average results for two treatments, even from the best-designed and executed experiment, is caused by chance, *i.e.*, by the operation of the uncontrolled variables included in "soil heterogeneity," etc., and not by the difference in the treatments applied. If the mathematical analysis of the data shows that the odds are 20 or more to 1 that this is not so, *i.e.*, that there is at least 95 per cent probability that the difference is caused by the difference in the treatments, then that difference is said to be "significant," and the "significant difference" is the minimum difference allowing these minimum odds.

Any difference between treatment averages which is less than this "significant difference" is "insignificant," *i.e.*, it is not safe enough to ascribe it to the difference in treatments, because the odds against its being caused merely by chance are less than 20:1. Anybody who is prepared to take more of a chance, is perfectly at liberty to do so. He may wish to accept 10:1 odds instead of 20:1, in which case a smaller difference is accepted as caused by the treatments under test.

As in other walks of life, the amount of chance one is prepared to take depends on what is at stake. To take action on a low probability may be worth the risk when the result of the gamble is big enough if it succeeds, but anybody doing so must admit that it is a gamble, involving risks bigger than the scientist is justified in giving his blessing to — even though he also may sometimes be forced to take them. When therefore a difference is stated by the scientist to be "significant," remember that it does not mean that it *must* be a real and not a chance difference, but only that the risk of its being a chance difference is so small as to be fairly safely

accepted. Similarly when the result is said to be "not significant," it does not mean that it cannot possibly be real, but only that the risk of its being only a chance difference is too big for safety.

WHEN SINGLE-PLOT EXPERIMENTS MAY HAVE SOME VALUE.

It was mentioned earlier in this paper that if the difference is big enough, even "single-plot" experiments can be relied upon. Thus if the yield of one plot is double that of the control, the odds are pretty high that this is chiefly the effect of the treatment, but if the difference is only 10 or 20 per cent, the odds are perhaps 5 or 10 to 1 *against* its being due to anything except chance.

There are two other factors which may greatly reduce the uncertainty of the results from single plots. The first of these is the *time factor*. If single plots have been carried on for a number of years, with careful recording of yields all the time, it may be possible by studying the relative behaviour of the plots during that time, to find differences in trend; the yield of one plot may have gone steadily up and that of another steadily down. Statistical analysis of such results is possible, and may often yield results of fair reliability. There is in fact replication here, though it is replication in time, not in space. But we do not want to wait for years to get definite results, if we can possibly avoid it, so whilst this is some consolation to those who have records of single-plot experiments started some years ago, it is no recommendation, and is not intended to be, that single-plot experiments should be laid down now.

The other factor is the *personal factor*. The planter who knows his rubber well can often see effects which have not or cannot be accurately recorded. It may not need more than a single plot to enable, *e.g.*, the effect of manures on foliage and canopy to be clearly seen. If an area in which the trees had small sickly-coloured leaves, thin canopy and obvious "stag heads" and die-back, develops in a thick dark green canopy of large well-formed leaves, with lots of a new growth in a few months after manuring, so that its improved condition leaps to the eye in comparison with the area not manured, then it would be sheer nonsense to deny, because only a single plot had been used, that the improvement was practically certainly caused by the manuring. Less spectacular differences in bark condition may also be detected with reasonable certainty by the man who knows his trees well. Should such improvements later be paralleled by the recording of a higher yield from the manured plot, then it is more likely that this is an effect of the manuring, even if it cannot be taken as sufficiently probable to satisfy the statistician.

We must remember that the statistician is the servant, not the master, of the experimenter, in much the same way that the architect is the servant, not the master, of the owner of a building. We expect the architect to design for us a building which is safe, and suitable for the use to which it is intended to be put, and so long as the building is properly constructed to that design, he takes the responsibility for its safety and suitability; but if we ask for a design for a dwelling house and he submits one more suitable for a block of offices, we do not have to accept it, whilst if he submits alternative designs, it is for us and not for him to decide which shall be accepted. We are perfectly at liberty to erect a building without any design or advice from an architect (that is, assuming we are outside the jurisdiction of the Municipal Authorities!) provided we do so on our own responsibility and do not ask him to accept any.

In just the same way, the experimenter is obviously quite at liberty to carry out "single-plot" experiments, and to draw his own conclusions from them, perhaps merely because he has a "hunch" that the result is so and so, or because he knows from his own observation that there have been effects which could not be recorded in figures, so long as he does not expect the statistician to take any responsibility for them, and so long as he does not expect others blindly to accept his conclusions. The rigorous mathematical tests which the statistician applies, on the other hand, serve as a guarantee which others can readily accept, and so they enable experimental results to be of use to the whole body of planters. But such tests can be validly applied *only* if the experiment is properly laid out in the manner already exemplified.

VARIATION BETWEEN LOCALITIES.

FIELD EXPERIMENTS AND SOIL SURVEYS.

It goes without saying that the results of an experiment in one locality cannot be unreservedly applied to another where conditions of soil, climate, trees, etc., are different. Due allowance must always be made, if it can, for such differences and it is here that the close relation between soil surveys and field experiments is of such importance. The two should always go hand in hand, for when the soils of a country have been properly mapped as the result of a soil survey, it is possible to decide how far results obtained from experiments on a given type of soil are applicable in other places, according as the soils in those other places resemble or differ from that of the locality where the experiment was carried out.

Until such knowledge of soils is available as a result of a soil survey, advice on rubber cultivation in localities where the soil has not been examined and experimented on must always be of less

sureness. The scheme of experimental planting under the ægis of the Rubber Research Institute, along with the soil survey work which is being done, is for that reason of the utmost importance, and may be expected to yield results of the greatest value to the rubber-growing industry of Malaya in due course.

LOGICAL SELECTION OF PROBLEMS.

There is another aspect of field experiments with rubber; that of the choice of treatments in the scheme of the experiment. This refers more particularly to manuring experiments. There is sometimes a tendency to jump ahead to the study of matters of secondary importance before those of overruling primary importance have been elucidated. Thus experiments have sometimes been laid down to test, for instance, which form of phosphatic manure is best — say rock-phosphate or super-phosphate — or what is the most effective amount to use, — before it has been found out whether on the soil in question there is need for any phosphate at all. The first essential question is, does the use of nitrogen, or of phosphate, or of potash, produce any result. Only if any one or more of these is beneficial, is it worth while comparing the relative merits of different forms of nitrogenous, or phosphatic or potassic manures as the case may be, or the relative effects of small and larger dressings and so on.

At the same time, in view of the relatively long time which must elapse in experiments with rubber trees before results of reasonable certainty are obtained, it is only natural that one should wish to avoid having to tackle the problems only in this logical order one after the other. An important recent advance in the design of experiments is one which enables many of these different questions to be answered together by an experimental layout which is not much more complicated than that which is needed to answer only one set of problems at a time. Such an experiment has the further advantage that it may supply important information on what is known as the "interaction" of factors.

No attempt will be made here to explain in detail what is meant by this "interaction," but the kind of thing it covers is well enough illustrated by the following example: A soil might be so very deficient in phosphate that plants could not make more than very poor growth on that account, and so long as no phosphatic manure was given, nitrogen manures had little or no effect. The use of phosphatic manures would have a marked effect, but the much improved growth or yield thus resulting might *then* be prevented from being still better because the soil was not too well supplied naturally with nitrogen. The use of a nitrogenous manure along with phosphate would then produce a pronounced effect, additional to that produced

by phosphate alone. That would be called a nitrogen-phosphate interaction, for the effectiveness of the nitrogen was subject to the presence or absence of phosphate. Nitrogen and phosphate together then give a bigger effect than the sum of the effects either gives in the absence of the other.

PRACTICAL ASPECTS OF FIELD EXPERIMENTS WITH RUBBER TREES

It is not possible to deal here with certain special aspects on the practical side of the conduct of experiments on rubber, such as the size and shape of plots, the effect of individual variations between tappers, the effect of the average tapping height, avoidance of interference or "poaching" between adjacent plots, and so on. There is however one important aspect to mention. Hitherto it has been customary to use plots consisting of whole tasks or at most half-tasks owing to the difficulty of organizing the tapping of smaller units without great uncertainty as to the reliability of the results. However, even when the results are obtained in the form of the yield of dry rubber from normal-sized tasks, it is often impossible on estates to supervise the collection and carrying-in of the latex by the coolies well enough to be reasonably sure that there has been no levelling-up of the amount of latex from different tasks. Such levelling-up will, of course, tend to vitiate the results of the whole experiment.

Another service which statisticians have done to field experimentation is the study and development of sampling methods for determining the yield of plots, instead of weighing the whole produce from them. Such methods should be particularly applicable to, and valuable for, experiments with rubber trees, when the annual yield of a given task is made up of perhaps 250 individual tree-tappings on each of, say 150 days, or 37,500 separate samples. Work is now being done at the Institute on this subject, and also by Dr. Haines of Dunlop Plantations, Ltd.

It may be expected that by collecting the latex separately from only perhaps one tree in every ten, or perhaps only one or two days a month, or less, considerably more accurate results from experiments will be obtained. Moreover, such a method would have the further advantage that individual plots in an experiment need no longer be as big as tasks or even half-tasks. Subject to their not being so small as to introduce extra troubles from individual tree-variation, they can be any size desired. For budded rubber in particular, where the uniformity from tree to tree is much higher than with seedlings, very considerable reduction in the size of plots may be possible.

CONCLUSION.

In spite of some digression from the original theme of the reason why the modern type of layout of field experiment is necessary, and how the statistician's services can be of enormous value to field experimentation with rubber, it is hoped that it has been convincingly demonstrated that the old idea of "single-plot" experiments, which still dies hard with many planters, is really of very little use. As to how the modern type of experiment — Randomised Blocks, Latin Square and the more complex type I mentioned a short time ago — do what they have been stated to do, this is a matter which can be properly explained only in terms of rather abstruse mathematics, and in this place it must suffice to refer to the notes and explanation appended to the samples above.

In conclusion, it is apt to cite the words of Robert Boyle, written in 1673, under the title of "Concerning the Unsuccessfulness of Experiments" as quoted by R. A. Fisher at the commencement of his book on "The Design of Field Experiments":—

> "I am very sorry, Pyrophilus, that to the many (elsewhere enumerated) difficulties which you may meet with, and must therefore surmount, in the serious and effectual prosecution of experimental philosophy I must add one discouragement more, which will perhaps as much surprise as dishearten you; and it is, that besides that you will find (as we elsewhere mention) many of the experiments published by authors, or related to you by the persons you converse with, false and unsuccessful (besides this, I say), you will meet with several observations and experiments which, though communicated for true by candid authors or undistrusted eye-witnesses, or perhaps recommended by your own experience may, upon further trial, disappoint your expectation, either not at all succeeding constantly or at least varying much from what you expected."

NOTES ON DRYER TEMPERATURES.

J. LAMB.

The following is an extract from a letter recently received which explains itself:—

> "While travelling around the country I noted most estates worked their tea driers, under all possible climatic conditions, at 180°Fah. and sometimes lower, whereas on other estates particularly where the Superintendents were keen teamakers, temperatures at their tea driers during recent wet weather varied between 195° and 200° Fah.
>
> "I was informed, perhaps erroneously, the T. R. I. had stated 180°Fah. was the best firing temperature under any conditions, but it is my opinion that a figure in the vicinity of 195° is more correct and teas so fired keep better for the London market."

The considerations governing firing temperatures were explained in very full detail in *The Tea Quarterly*, Vol. VIII (1935), page 43, under the heading "The Theory and Practice of Tea Drying."

For reasons which will be obvious after reading this article, we have avoided hard-and-fast statements about inlet temperatures because any figure we give can never be of universal application. We have, however, made very definite statements about exhaust temperatures and we have not found any reason to alter these; on the contrary, practical experience gained from advisory work and the result of our investigations on enzymic fermentation have strengthened our views. In case there is any misconception on this point let me repeat that continual working with very low exhaust temperatures (below 120°F.) or very high exhaust temperatures (above 135-140°F.) will sooner or later give trouble with keeping quality. When low inlet temperatures (below 190°F.) are employed, the upper limit may be raised slightly.

A warning is necessary in this connection where tilting tray dryers are concerned and should be noted especially by those using Colombo dryers. The exhaust temperature measured in the fan casing of a suction dryer or in the exhaust vent of a pressure dryer may fall as low as 110 or even 105°F. for a few seconds immediately after the bell has rung and the top tray tilted; in this case the temperature passes through a regular cycle approximately every three

minutes if the drying time is 20 minutes. Immediately after the top tray has tilted the temperature falls to 105-110°F. rising quickly to 120-125°F. and then slowly to 135°F. before the bell rings again.

This cycle can only be observed with a sensitive thermometer. A thermometer with a heavy brass case will lag behind the true reading and may remain fairly steady between 120 and 130°F. depending on the thickness of the brass case.

The question as to the best inlet temperature cannot be so definitely answered, since this must vary to some extent according to the particular needs of individual estates. As an example I will quote the case of the estates where a touch of high fire amounting to a distinct maltiness in the fired tea is desired; in this case a temperature of at least 200°F. will probably be found by actual trial to give the best results.

In other cases, nowadays fortunately rare, estates are forced to use temperatures of 210 and 220°F. in order sufficiently to raise the drying capacity of their machine to cope with a large crop.

Quite recently very low firing temperatures of the order of 165-170°F. have found favour in certain quarters. In this case the exhaust temperature must still be maintained above 120°F. and to do this the time of firing has to be increased and special pulleys may be found necessary. If a very low temperature is adopted a period of greater than 25 minutes, which is the longest time possible with pulleys normally fitted to firing machines, is required for satisfactory drying. Firing at low temperatures is thermally less efficient than firing at normal temperatures and this raises the cost of firing.

Having qualified statements on the question of firing temperatures as explained above, we can however say that in the majority of cases an inlet temperature of 190 to 195°F. gives very satisfactory results with a firing period of approximately 20 minutes. When other methods are employed for some definite reasons, suitable modifications have to be made in the drying period and thickness of spreading. The limits of exhaust temperature should, however, remain unaltered.

Inlet temperatures should be measured immediately below the bottom row of trays. It is a common complaint when a machine is fitted with two thermometers at the hot-air inlet, that the readings do not agree; this is usually because one is fitted towards the stove end of a longish duct and the other below the trays. When speaking of inlet temperatures the point at which they are measured should be stated.

To maintain the correct exhaust temperature the rate of spreading should be adjusted from time to time, particularly when changing over from dhools to big bulk. In the fully automatic dryer, especially the modern pressure type, this adjustment is merely a matter of a few turns of a handle and may be carried out by the firing cooly.

Adjustments to the rate of spreading are more difficult to carry out in a tilting tray dryer and may be left to the discretion of the Teamaker, who should ensure that the exhaust temperature is of the correct order. In this case the Teamaker should give instructions for increasing or decreasing the loading by a half to one pound at a time according to whether the temperature is too high or too low. Such instructions are simple and easy to carry out and will save a good deal of trouble when the withers are too hard or too soft, which variations are bound to occur from time to time in even the best regulated factories.

Big bulk is better fired at a slower rate than the dhools, and a thicker rate of spreading will compensate for the rise of exhaust temperature which takes place under these conditions. At St. Coombs we thicken the rate of spread fairly considerably and change to the slowest pulley when the big bulk reaches the third row of trays. There is invariably a slight rise of 5-10°F. in the exhaust temperature, as it is impracticable to thicken the spread to such an extent as to maintain the same temperature as when firing dhools. The big bulk however comes out fully fired and the dhools fired immediately before or following the big bulk are not over-fired. If dhools follow the big bulk, the pulley should be changed again when the dhools have reached the third row of trays. The slowest pulley on almost all automatic machines causes the leaf to pass through in about 24-25 minutes while the next pulley on which dhools are fired corresponds to 20-21 minutes.

To ensure evenness of firing it is desirable that temperature and loading should be even, and it is a sound practice to check these points occasionally. Sensitive long stemmed thermometers are best for this purpose and three of these instruments each reading up to 220°F. (they are not expensive, they cost 3/6 each in England) will be required. In an endless chain dryer they should be suspended at the two sides and the centre, across the top row of trays, at about the mid point of the length of the row of trays. Most tilting tray dryers are provided with two thermometers, one on either side of the drying chamber but, for purposes of checking, the test thermometer may be inserted through one of the bolt holes in the flange of the dryer thermometers if desired. If test thermometers are used in this manner, great care should be taken to ensure that the thermometers on either side project for equal distances into the air stream.

When the machine is empty, all thermometers in the *same part* of the machine should read alike to within a few degrees, so in the case of the Endless Chain Dryer the three thermometers suspended above the top trays should give equal readings. The expression *same part* really refers to position in the air flow because even when the machine is empty the air will cool to some extent on the way through, owing to losses by radiation and conduction of heat.

If the temperature distribution does not appear to be even, further investigation is necessary and further advice may be required, but before this is sought the temperature under working conditions should be checked.

Provided temperatures are fairly even when the machine is empty they should be even under working conditions; if they are not, a fault in spreading is indicated and the drying will probably be uneven. The three thermometers suspended above the top tray in the case of the automatic machine should all give similar readings and in a tilting tray machine, of the pressure type, even temperatures should be found across the exhaust vent. Conditions in the suction type of tilting tray dryer may be checked by boring small holes in the two fan ducts at corresponding positions and inserting the thermometers (equal distances) through these, taking great care to ensure that cold air is not sucked in round the thermometers while readings are taken. For the latter purpose the thin long stemmed sensitive thermometers mentioned above are essential.

To ensure even distribution of leaf, and therefore even drying in a tilting tray dryer, it is sound practice and quite easy, to spread the top tray in two halves. Instead of having one box to hold say 10 lbs., two boxes each holding 5 lbs are used and the leaf from these spread separately on the two halves of the tray. The centre of the top tray may be actually indicated by marks on the iron work. Such a procedure is essential when carrying out tests and should be made a regular practice for the best results. If these tests reveal uneven conditions, advice should be sought without delay.

Whilst discussing firing temperatures it is pertinent to draw attention to the fact that the inlet temperature should be maintained as steady as possible. This is an obvious necessity which is often overlooked and, save in very few cases, there is no real difficulty in maintaining a temperature to within, at the very utmost limit, 5°F. on either side of the temperature aimed at. Fluctuations are almost entirely due to faulty manipulation of the heater which subject was dealt with very fully in a series of articles in *The Tea Quarterly* in 1936 under the heading of "Fuels for Tea Dryers." Inlet

temperature is easily regulated by means of the *flue* damper unless there is some component in the heater badly out of order.

Cold air ports should only be used as an emergency means of controlling temperature. Even in a pressure dryer where the air is bulked by the fan before being forced into the drying chamber, the introduction of large quantities of cold air may prove too much for the bulking capacity of the fan, especially when the fan is fed by two separate ducts on the suction side. In a suction dryer cold air can never be expected to bulk with the hot air it is desired to temper. The hot and cold air simply flow through the machine in separate streams, leading to very uneven drying conditions. Cold air allowed to enter by the side ports on the drying chamber of some suction dryers merely cools the thermometer!!

Any discussion of dryer temperatures is incomplete without some reference to the question of the moisture content of fired tea. Here again the question is often asked "What do the Tea Research Institute recommend." Although we can be more definite on this point our recommendation should be used with reason. Apparatus for the determination of moisture content is now easily available and perfectly simple to manipulate, making it possible for each estate to work out the finer adjustments made necessary by their own particular circumstances.

In general we say it is *safe* to pack tea for despatch at 4 to 5 per cent moisture content. If more suitable packing materials are discovered in the future, it may be possible to allow another one or two per cent of moisture with *safety*. If Kraft paper had not been rejected on the grounds of absence of scrap value (see Bulletin No. 18, pages 17 and 79), this class of material would probably have afforded sure protection for tea at 6 per cent moisture content which would be an important consideration in times of unrestricted output.

Most estates accept the figure 4 to 5 per cent for packing, but question the best figure for tea discharged from the dryer. In the first case most estates fire to a much lower moisture content than generally thought so I will outline the procedure for the determination of moisture in teas freshly discharged from a dryer.

1. Select a number of ordinary sample tins with well-fitting lids.

2. Take a sample by placing one of the tins under the discharge of the dryer and allowing the tea to fall into it. Replace the lid immediately sufficient has been collected, making sure that small pieces of leaf are not jammed between the lid and the tin.

3. Allow the sample to cool. 15 minutes should suffice.

4. Weigh out 10 grms. of the sample on the balance and transfer to another of the selected sample tins with a camel hair brush. This operation should be performed on a white glazed tile so that any scattered pieces may be observed and collected. Great care is necessary as any fragments lost will cause an erroneous result on the high side.

5. Place the sample in the oven (the lid must of course be off the tin) and allow to remain $3\frac{1}{2}$ to 4 hours. The temperature of the oven, if electrical and of the hot air type, should be adjusted to within a few degrees of the boiling point of water at the particular elevation.

6. When the sample is removed from the oven the lid should be placed on the tin and the sample thus allowed to cool before weighing. 15 minutes should suffice.

7. The sample should then be carefully transferred to the pan of the balance observing the same precautions as in 4.

My attention has frequently been drawn to the use of the Silica Gel Dryer usually provided with the balance and which should be kept in the balance case. The method of using this is as follows:—

1. At least once a week, or when the paper indicator changes colour, place the Silica Gel in the oven and allow to remain at the same temperature as that employed for drying samples for 3-4 hours.

2. At the end of this period remove the Silica Gel from the oven and place the lid on the tin.

3. Allow to cool with the lid on. 15 minutes should suffice.

4. Remove the lid from the tin containing the Gel, place it on the underside of the tin so that it does not get lost and place the whole apparatus in the balance case. The paper indicator if available may be placed upright by sticking the pin in one of the holes in the perforated aluminium component.

When determining the moisture content of any other sample the procedure from 4 onwards should be followed. If these methods are adopted, an exact idea may be formed of the actual "dryness" of the tea discharged from the dryer. Samples should periodically be removed from the two sides and the centre of the discharge (or the discharge tray in a D.D. Sirocco) to check the evenness of the moisture content.

In most cases firing to approximately 3 per cent of moisture gives satisfactory results and if the bins are in good condition and facilities available for drawing hot air around the bin space, the tea may be graded, bulked and packed without recourse to final firing. Some estates prefer to final fire whatever the moisture content or season, in which case a higher moisture content in the fired tea may be practicable.

In some factories, particularly those with a small crop which necessitates long storage periods in the factory bins, and those with bins in a damp situation, or of poor construction, it may be found necessary to fire to a low moisture content initially and then to final fire before packing. Generally speaking, those factories in districts affected by the South-West Monsoon need to adopt special measures during very wet weather when damp air pervades the whole factory and dryers are taxed in raising hot air for withering.

Obviously therefore each estate must study its own special requirements; a relatively simple matter when apparatus is available for moisture content determination.

Many people have a prejudice against firing to a very low moisture content, although as stated above most estates fire to a lower moisture content than realised as I frequently find less than 2 per cent moisture in freshly discharged teas.

We are carrying out accurate experiments on this matter and will publish the results as soon as they become available. Although at first sight this appears a simple problem it does in fact involve many considerations which all have to be worked out separately. Low moisture content in the tea discharged from a dryer may be a result of:—

1. A hard wither.
2. A high firing temperature.
3. Very light loading.
4. A long firing period.
5. Various combinations of the above factors, of which there are $4 \times 3 \times 2$ combinations $= 24$ in all.

Low moisture contents may not necessarily be the cause of poor results; it may be merely symptomatic, the real cause of the poor results being, in fact, something like a combination of high temperature and long firing period, which would give equally poor results whatever the moisture content. I quote these circumstances as an example only, and it is intended as a warning against highly erro-

neous conclusions which may be arrived at by experiments of a cursory nature. Season may, in addition, affect results of firing experiments.

In actual practice I have come across teas containing only 0.5 per cent moisture discharged from dryers on up-country estates getting very good results, so I hesitate to quote a figure for the lowest moisture content to which tea should be fired. However, 3 per cent is safe and satisfactory in most cases, so this must serve as a rough guide until further accurate information is available. It is obviously uneconomical however to dry tea to a lower figure than is necessary.

In conclusion, we offer to check up results of moisture content determinations carried out on the estate if the samples are packed as described in *The Tea Quarterly*, Vol. VIII (1935), page 59. In the few cases where the estate cannot afford the cost of apparatus for determining moisture content, we are prepared to do a limited number of determinations when definite problems arise.

MARKETING OF CEYLON TEA.—I.

COLOMBO MARKET—THE BROKER.

Of the many links in the chain between tea on the bush and tea in the pot, the Broker link marks the dividing line where tea passes from selling to buying interests.

The Broker's primary duty of course, is to sell the tea and to sell it at the best possible price.

His second duty is of an advisory nature — reporting on teas, keeping Agents posted regarding market requirements, etc.

Thirdly, he is responsible for the smooth running of the machinery of transfer.

About half the tea produced in Ceylon is sold in Colombo, and of this quantity nearly all is dealt with through the weekly auctions held at the Chamber of Commerce Sale Room.

Particulars of teas for sale are received by brokers up to 5 p.m. on Friday for the sale of the following Tuesday week. It is probably generally known that the minimum quantity that can be printed is

300 lbs. of Fannings or Dusts and 500 lbs. of other grades. Catalogues are then made up and printed, samples drawn and distributed to the Trade.

Some 200 catalogues are distributed; over 30 buyers receive a sample of every tea up for sale and many others samples of a limited number of lines in which they may be interested.

The drawing and distributing of samples, although routine work, requires careful supervision as mistakes would be serious.

Samples are then tasted and valued by the brokers and discussed with the Agents concerned before the sale.

At the Auction therefore the Broker has before him a valuation agreed between himself and the Agent; unfortunately, however, market conditions are not ruled by brokers and sellers' valuation, and during the sale auctioneers have to make a very rapid revaluation, either up or down, to suit the exigency of the situation. Generally speaking, it pays to meet the market in the sale-room, but there are occasions when certain lines appear to be relatively cheap and these are withdrawn, usually with a bid registered, in the hope of being able to improve on that bid before it expires at 1 p.m. the following day. This is only advisable when the *relative* value is low as competition after the sale is limited, most buyers having filled their orders.

After the sale therefore, there are the "bid" lots to be improved on if possible and the "no bid" lots to be sold, Contracts, Delivery Orders, sub-Credit Notes (Coupons) and purchase samples to be issued, and proceeds to be collected and paid out.

Then the weekly cycle starts again, or rather overlaps with the next catalogue to print and value.

The market situation in Colombo varies slightly from London. The biggest difference is probably that teas are paid for 5 working days after sale, whereas in London the system is 3 months terms, with discount at 5 per cent per annum on earlier payment.

In Colombo buyers attend the sale and bid direct.

In London the buying is done through second hand brokers or the selling broker at $\frac{1}{2}$ per cent commission.

The number of buyers in Colombo is much smaller than in London, but as one buyer may represent clients in the United Kingdom, America, Australia and South Africa the interest is widespread. A rather peculiar difference in the markets is the large quantity of tea in Colombo Auctions packed in momi chests, whereas

London is almost entirely patent packages. This is rather a relic of the past as Colombo buyers now generally prefer patent packages.

Sampling methods are different. In London all tea goes into Customs Bonded Warehouses and is re-weighed and sold on warehouse weights. Buyers send down to the warehouses for their own samples, which are drawn from "show" packages.

In Colombo teas are stored at Agents' Stores, and samples are drawn and distributed by the selling broker.

A COLOMBO BROKER.

REGENERATION OF TEA.*

M. C. EVANS

Much discussion has recently taken place regarding the improvement of tea areas by the replacement of uneconomic bushes and other special treatment and the desirability or otherwise of replanting complete areas as opposed to other methods of renewing the *product*.

During the period 1930 — 1932 the writer had occasion to convert some 15 acres of old Rubber into Tea, replanting 5 acres yearly, and opportunity was taken to plant a Contour Clearing with plants spaced $1\frac{1}{2}$ feet $\times$ 5 feet. This was planted with germinated seed along deep forked contours, without holing. The total cost, excluding the removal of the Rubber, worked out at Rs. 160 per acre for the first year, and the total cost up to June, 1937 amounted to Rs. 510 per acre. The clearing had to face the drought of 1934, and, owing to the depression, no manure was available until 1934.

Despite these difficulties, the appearance and yield of this small clearing is such as to encourage replanting by this method elsewhere rather than any other form of rejuvenation.

Since, however, in any commercial undertaking, figures are usually required in support of any proposal, the following is an attempt to compare two methods of rejuvenation from the point of view of cost.

* The Institute does not necessarily endorse the views expressed in papers contributed by persons other than members of the Staff.

Firstly, however, the object of rejuvenation is to eradicate the uneconomic yielder and replace with something better, in other words, to maintain, or increase the yield of the estate by adding to the number of economic yielders.

It is generally conceded that, within reasonable limits, the larger the number of bushes per acre the larger the yield of that acre. If this be so, then it necessarily follows that the same is applicable to the estate; the larger the number of economic bushes, irrespective of where they may be on the estate, the larger the yield must be. The object to be aimed at, therefore, is to increase the number of economic bushes on the estate in the cheapest and most effective manner possible. It is submitted that it is the cost per plant planted that matters, not the cost per acre, or the acreage treated. For instance, it will be more productive to establish 20,000 new plants over $3\frac{1}{2}$ acres for Rs. 1,000 than to establish 100 new plants per acre over 100 acres for Rs. 10 per acre.

The two methods to be compared are as follows:—

METHOD A.—To select an area on the estate containing the largest percentage of uneconomic yielders, eradicate all tea entirely and replant on the contour system, say $1\frac{1}{2}$ feet × 5 feet, thus establishing approximately 6,000 plants per acre in place of the original 4,000 old bushes.

METHOD B.—To eradicate all uneconomic bushes over the whole estate, and to supply all vacancies by the ordinary supplying methods, cutting large holes, protecting all plants, and applying extra manure to supplies for three years after planting.

For the purpose of comparison let it be assumed that the estate has an original stand of 4,000 bushes per acre, and that the percentage of vacancies and uneconomic bushes combined amounts to 14 per cent. Let us also indulge in the delightful assumption that every plant planted is a success! Further for the sake of the comparison, figures and acreages are taken on which estimates have been worked out.

The estimates are based on actual work done in the past, with the exception of the replanting estimate; this is based on the cost of the ex-Rubber converted clearing mentioned above, and may therefore be inaccurate. Those, however, who disagree with these figures can substitute their own and work the comparison accordingly.

COMPARISON.

METHOD A.—Eradication of the old bushes from the complete area. 6 acres are replanted at 6,000 plants per acre. Thus 36,000 new plants are put out.

Cost at Rs. 200 per acre ... Rs. 1,200

This cost of Rs. 200 per acre represents only the bare cost of eradicating the old tea and lining, forking contours, and planting the new plants.

Other routine cultivation and work, such as Pruning, Manuring, Forking, Draining, Weeding, etc., would be undertaken if the area were not being replanted, and this cost of Rs. 200 per acre therefore represents for the purpose of comparison only the *extra* cost involved by replanting.

Regarding the upkeep of the new area, the same holds good; normal cultivation costs, sufficient for the upkeep of the new area until it comes into bearing, would normally be incurred and therefore for the purposes of this comparison may be disregarded.

Since Tea Export Control is in operation, it should be possible to harvest the quota of this small area replanted from other areas; in other words, enhanced profit from larger harvests from other areas should balance the loss of crop from the replanted area.

During Export Control then, loss of profit need not occur. Total actual cost at the end of three years therefore remains at the initial figure of Rs. 1,200.

METHOD B.—Taking a field of 40 acres, allowing a normal stand of 4,000 bushes per acre, and vacancies and uneconomic bushes totalling 14 per cent.

The number of vacancies then after eradication totals in round numbers 22,000.

The planting of 22,000 supplies will cost ...	Rs.	1,451
Also, in *addition* to the normal cultivation programme of that area they will require manuring annually for three years at 2 oz. per plant, say	,,	550
Application of manure, pegging, after care, etc., for three years, say	,,	400
Total for 22,000 supplies ...	Rs.	2,401

The comparison now stands as follows:—

METHOD A.—36,000 plants after three years cost Rs. 1,200

METHOD B.—22,000 plants after three years cost ,, 2,401

Reducing this to cost per plant:—

METHOD A.—Costs per plant planted ... 03.33 cents

METHOD B.—Costs per plant planted ... 10.91 ,,

But it will be observed that the comparison is incorrect in that plants planted by Method A do not all represent additional plants as is the case in Method B.

Examining this point it appears that in the six acres dealt with in Method A there were 14 per cent of vacancies and uneconomic bushes — though in actual practice the worst area would be selected and the uneconomic percentage would be higher here than elsewhere.

Allowing for a stand of 4,000 bushes per acre, vacancies at 14 per cent then account for $\frac{4,000 \times 6 \times 14}{100}$ or 3360.

Thus before the area was cleared there were $(4,000 \times 6) - 3,360$ or 20,640 yielders.

Therefore in this area the additional plants now number 36,000 −20,640 or 15,360 plants.

Comparing costs again on this basis:—

METHOD A.—Costs per additional plant planted 07.8 cents

METHOD B.—Costs per addition plant planted 10.9 ,,

Method A has moreover numerous other advantages, amongst which are:—

1. The soil erosion problem is largely solved.
2. A considerably larger percentage of successes is probable in practice with Method A.
3. Method A not only provides the additional new plants required, but also over the area treated replaces the old product with young and improved material, thus obviating the possibility of annual wastage from old age, a contingency likely in Method B.

It is suggested therefore that, granting that the object of rejuvenation is to increase the number of economic yielders on the estate and given that a certain sum of money is available for that purpose,

replanting over a proportion of the acreage may be not only the most effective, but also the cheapest method of achieving that end.

It will be realised that the higher the percentage of uneconomic bushes the more cogent the argument in favour of replanting becomes.

A criticism of the foregoing may be that the estimate of replanting at Rs. 200 per acre is unduly low.

In the converted clearing mentioned above, actual cost of seed, lining and pegging, forking contours and planting, shading and watering, amounted to Rs. 121 per acre.

In this estimate of Rs. 200 is included:—

Rs.	70 per acre	for eradicating old tea
,,	80 ,,	for one maund seed at Rs. 36, nursery, forking contours and planting.
,,	30 ,,	for sowing and control of ground cover.
,,	20 ,,	for shading and watering.
Rs.	200	

Germinated seed would be planted in pairs to allow of a certain amount of selection.

One of the problems of the practical planter is how to choose best between the prohibitively expensive ideal and that which is uneconomically cheap; in other words, how to obtain the optimum value per unit of expenditure. It is not claimed that the method outlined is necessarily the best method by any means, but it is claimed that under difficult conditions it has succeeded in the case of conversion from Rubber to Tea. Rubber, we know, is the dominant partner when interplanted with tea; if this method has succeeded in old rubber land, there seems no reason why it should not be equally successful in old tea land. It is at any rate worth trying.

THE CHEMISTRY OF TEA.

J. LAMB.

It was recently announced that the Board of Control of the Tea Research Institute of Ceylon proposed to set aside a sum not exceeding Rs. 5,000 which they are prepared to contribute to a scheme for a special investigation of the chemistry of tea.

This scheme is dependent on the active co-operation and support of other Institutes carrying out research on tea problems, and it is hoped that it will be possible for the investigation to be entrusted to an eminent chemist such as a University Professor of Chemistry who has the facilities for such work to be carried out in his own laboratories under personal supervision.

Since it may not be apparent to all interested in the Tea Research Institute why such a scheme is necessary, it is proposed to outline our present knowledge of the chemistry of tea, and to explain what we hope to gain from the expenditure.

The Chemistry of tea as a subject does not lend itself readily to popular treatment and it is to be hoped that non-technical readers will not abandon their efforts to read through this article as soon as they come upon long names, and that the few technical readers will realise the limitations imposed and refrain from using the writer's own words as evidence against him should he ever publish papers under the same caption in technical journals. The main object of this journal is to keep the Ceylon Tea Industry in touch with the activities of the Institute and it is in that spirit that the article is written.

Generally, there is very little misconception, popular or otherwise, about the principal ingredient of the brew prepared from tea leaves. Almost every tea drinker is aware that tea contains a substance termed "tannin." The more timid members of this class have vague notions that it causes hob-nail liver or a leather stomach, while aggressive reformers who are always prepared to fear the worst about the effects of almost any food, except the most unpalatable, speak glibly of supposed ill-effects which arise from partaking of a liquid which in their view should be prohibited outside tanneries. The basis of these assumptions, for they are no more than assumptions, is the

ill-informed association of tea tannin with substances used for the production of leather. Terms of a general nature such as this word "Tannin" are very misleading at times, and even to people with specialised knowledge a loose term is apt to be very confusing unless they have personal experience of the particular group of substances which the term indicates. Thus, many people ranging from ill-informed neurotics engrossed in their digestive troubles to scientific and technical persons who would have been better served by a more accurate name, have fallen into the snare laid by the use of the unfortunate word "Tannin" probably derived from a Celtic word meaning "Oak" and later adopted to describe the substances (cf. tan liquor) and occupations (cf. tanner) concerned with leather making.

There are wide differences between different tannins in the same way as there are very marked differences in the properties of many substances included in the Alkaloid Group. A medical man would never confuse Strychinine with Morphine or Quinine although they are all members of the Alkaloid Group and have common chemical reactions which justify their being grouped together in a classification, yet the "Tannic Acid" of the Pharmocopoeia is not always clearly distinguished from tea tannin which is very different in chemical composition and properties, although both precipitate common reagents and have other common chemical reactions which justify their inclusion in the "Tannin" Group. If some other word had been used for the "Tannin Group" in the same way as "Alkaloid" has been used for the group which includes Quinine and Morphine, the ridiculous idea of tea turning the stomach wall into leather would probably never have been conceived.

Tannins as a class of substances have received very scanty attention compared to other well-known groups such as Alkaloids, Carbohydrates, etc., while the members of this group which occur in tea have not even had their proportionate share of attention as individuals of a very large class. Most of the work done on tannins has been devoted to the Gallo-tannins which are actually used in leather making; thus Nierenstein in a book of three hundred pages on the natural organic tannins devotes barely one page to Tea Tannin and says "The Chemistry of the Tannins is full of contradictions but in no special case are the contradictions so pronounced as in the case of the Tannin present in *Thea chinensis*, Sims., *Thea sinensis*, L., *Camellia Thea*, Lk., *Camellia theifera*, Griff. and *Thea Assamica*, Mast." To some extent the lack of progress with the chemistry of tea has been due to the lack of workers in this particular field which for some reason has not attracted attention from research workers on organic chemistry. In addition to this relative lack of

attention the somewhat complex and difficult nature of the tannins has also tended to impede progress, so we must appreciate the efforts of those workers who have given us the information we have at present and remember that future work will become less difficult as a result of the information they have made available. It is quite conceivable that once attention has been drawn to the relatively unexplored realms of the chemistry of tannins, as a result of work subsidised by the Tea Industry, a number of Academic Chemists may be tempted from the well-trodden paths in the realms of Plant Pigments, Alkaloids, Carbohydrates, etc. Although Industrial and Commercial Chemists are apt to gibe at purely academic work, we must all admit that it has provided the foundation and the framework of our knowledge. Nothing could be more helpful than general academic interest in the Tannins as a class.

We cannot consider in detail the whole of our present information or mention the names of all those whose have contributed to it. To Shaw, late of the South Indian Research Station, must, however, be given credit for attempting to distinguish the Tannins of tea by introducing the word "Theotannin," thus taking the first step forward enabling a distinction to be drawn between the substance of the tea leaf and others such as tannic acid of the pharmocopœia which is used for the treatment of diarrhoea, and as a coagulent in cases of burns, or from a variety of other substances used for tanning and for the manufacture of ink.

Shaw carried out a good deal of work on the separation and purification of the tannin substances of the tea leaf, also, with the aid of Jones, on the analytical estimation of Tannins as a group of substances occurring in tea. These researches extended the earlier work of Deuss and others.

Until quite recently the idea of Theotannin as a group of substances of the tannin class does not appear to have been actively entertained. Vague references have been made from time to time to inherent possibilities but very little active work on this aspect was undertaken until quite recently. The work of Russel in an English University Laboratory who made a study of the tannin in Hemlock, gave impetus to an investigation of the tannin substances of Formosan tea leaf, with the result that an amorphous tannin has been synthesised by Oshima who claims that his synthetic product is identical in all its properties with the amorphous tannin isolated from Formosan tea leaf. Oshima also states that this synthetic tannins closely resembles the tannin substances isolated by others including Deuss, Shaw and Tsujimura, but differs in purity.

There is an urgent necessity, therefore, for a repetition of this work to ascertain whether the conclusions apply equally to Ceylon tea leaf.

Shaw claimed to have separated some crystalline tannin from his preparation which indicates the presence of at least one other tannin in South Indian tea leaf, while Tsujimura isolated a substance of the tannin class which she claimed to be the gallic ester of a catechin. Previously she had investigated this catechin and called it tea catechin. In 1936 Deijs and Dijkman of the Java Tea Research Station published work confirming Tsujimura's findings and thus indicating that one tannin in Java leaf corresponds to a tannin in Japanese leaf, alhough Oshima was unable to detect the presence of this particular substance in Formosan leaf. Shaw was also unable to confirm Tsujimura's findings, while at St. Coombs we repeated the work of Deijs and Dijkman during 1937 and came to a conclusion in agreement with Oshima and Shaw.

Sufficient has been said to indicate differences in the chemical composition of the tannin substances occurring in the leaf of different countries. It is not unreasonable to suppose that there will be differences within Ceylon according to elevation, district and possibly season. The mixed jâts of Ceylon may also complicate our problem. Besides the true tannins characterised by their ability to precipitate gelatin, there are other rather simpler substances present in tea leaf which belong to the same class of organic compounds as tannins, but differ in that they do not precipitate gelatin; these substances are called catechins.

There are several different catechins which may occur in tea. The catechin mentioned above as Tea Catechin and isolated by Tsujimura has been the subject of a long controversy. Oshima has isolated considerable quantities of a catechin called Gallo-catechin from Formosan tea leaf which substance is very closely related to the amorphous tannin.

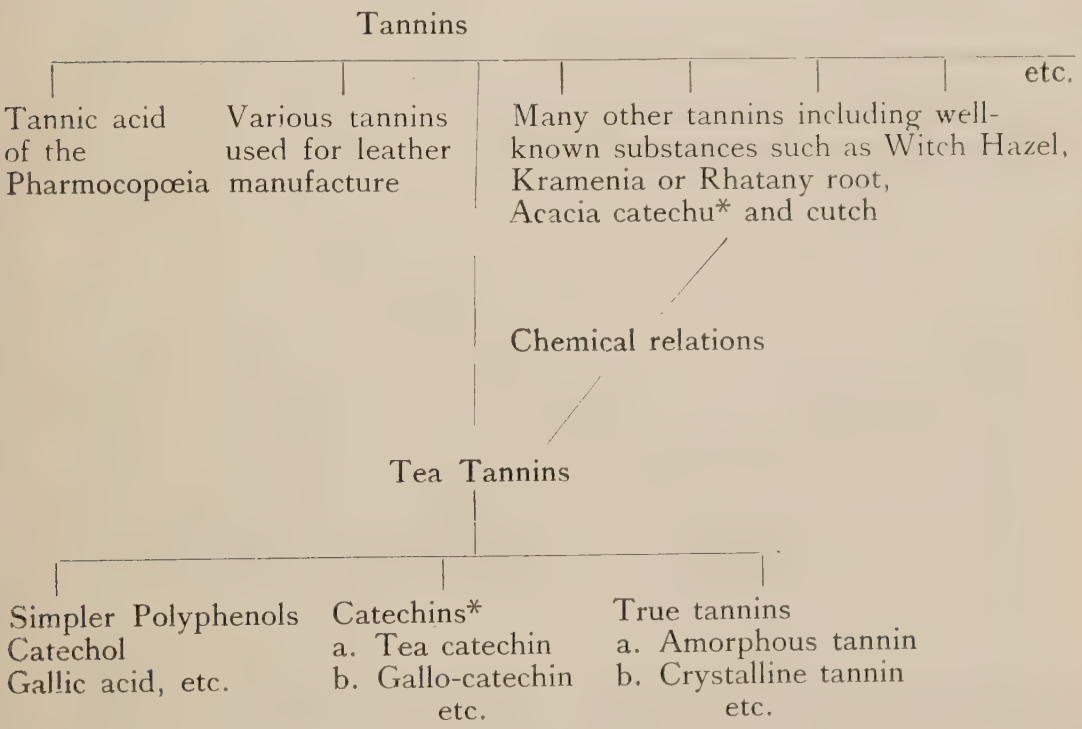
At St. Coombs we have isolated Gallo-catechin from Ceylon tea leaf by following the method of Oshima.

Of the class of compounds which we have in general termed Tannins we need only consider one other group which we will refer to as simple Polyphenols. In case of any possible confusion to the non-technical reader we will recapitulate to the extent of setting out the classification which we have adopted for the purposes of this article. To the substances of the tannin group which occur in tea we have given the general name Tea Tannins or (after Shaw)

Theotannins. Three sub-groups of tea tannins were then described:

1. True tannins.
2. Catechins.
3. Simpler Polyphenols.

In order to give a picture of Tea Tannins in relation to other tannins with which they are confused we will draw up a family tree as follows:—



Further description of the tannin substances in tea leaf will only serve to confuse, so the more practical aspect of the proposed investigations will now be considered. During fermentation which starts immediately the juices of the tea leaf are exposed to air, the tannin substances are changed to coloured compounds which are soluble in hot water and contribute the major part of tea liquors. The practical man knows quite well that different kinds of leaf, differences in the manner of expressing the juices of the leaf, and differences in the manner of fermentation, all influence the production of coloured substances. As well as the colouring matter which

* One of the Acacia catechus is probably identical with a catechu occurring in Tea. Acacia catechu which is separated during the manufacture of Cutch is used for chewing with betel, etc.

is soluble in hot water and dissolves into the liquors, there is insoluble pigment produced during fermentation which remains in the infused leaf. From the importance attached to the colour of these insoluble pigments by tea tasters we infer a close association between the production of a desirable colour in the infused leaf and desirable properties in the liquors.

These changes in the tannin substances are brought about by enzymes which are themselves non-living organic compounds. During the life of the leaf they take part in the living processes of the cells and in death they assist in processes of decay. Enzymes may be differentiated by their reactions, and in tea manufacture we are mainly concerned with those causing the cells to become permeable during the process of withering and the comparatively limited number causing the changes in the tannins during fermentation.

The position is therefore, in short, as follows:—

The tannins, catechins and other polyphenols, as a result of enzymic action, give rise to both soluble and insoluble coloured pigments, which are responsible for the colour of liquors and infusions. Besides colour in liquor these substances arising from the tannins of the tea leaf are mainly responsible for strength and pungency. We therefore wish to know the exact nature of each of the tannin substances present in green leaf and also their relative amounts under different conditions of elevation, season, jât, etc. It should then be possible to elucidate the problem of fermentation and the part played by each of these substances in the production of colour, strength, pungency, and also to find the connection between good liquors and good infusions. It is quite conceivable that the optimum conditions of time and temperature vary for each individual substance, in which case those responsible for the most desirable characters would receive the closest attention. A knowledge of the causes of dullish brown or persistently greenish infusions, and of the causes of greenish bitterness in liquors, would also be of immense practical value.

We have by no means wandered into the realms of idle speculation for at St. Coombs we have already studied the effect of tea enzymes upon certain individual tannin substances, and have succeeded in isolating products of the reaction. Russian workers have studied the same problems from a slightly different angle by adding various polyphenols to fermenting leaf. Their results are very interesting indeed so a table from a paper of Kursanov is reproduced.

Added Polyphenol	Intensity of Colouring of Infusion* % Standard.			
	Test 1	Test 2	Test 3	Tint of Infusion*
Control (water)	31	28	27	Reddish brown (Normal)
With guaiacol	41	29	40	Red mixed with brown
,, pyrogallol	46	48	41	Dark brown
,, gallic acid	50	55	44	Dark brown with grey tint
,, phloroglucine	19	21	16	Yellowish mixed with green

* The word infusion in the translation from the Russian refers to the soluble pigment, *i.e.*, the liquor.

Comparing the intensity and nature of the colour produced when phloroglucine was added, with that of the control when water alone was added, it is of interest to note that production of colour was actually inhibited and that the liquor assumed a greenish tint. It is not at all improbable that reactions of this nature may take place in actual practice at certain periods of the year. The writer has come across cases where all efforts to eradicate greenness of liquor have failed, and where such difficulties have disappeared apparently of their own accord when the type of leaf changed.

A good deal of information is available about the enzymes of tea as a result of work carried out in almost all tea producing countries. It is therefore possible to end this section dealing with Tea Tannins on an optimistic note, for it would appear that the proposed scheme for research on the chemistry of tea, which should not meet with any serious difficulties now that money has actually been provided, will furnish information that will enable rapid progress to be made in practical tea manufacture.

MINUTES OF A MEETING OF THE BOARD OF THE TEA RESEARCH INSTITUTE OF CEYLON HELD 21-4-38.

Minutes of a meeting of the Board of the Tea Research Institute of Ceylon held at the Grand Oriental Hotel, Colombo, on Thursday, April 21st, 1938, at 2-30 p.m.

Present.—Mr. James Forbes (Jnr.) (Chairman), the Hon'ble the Financial Secretary (Mr. H. J. Huxham), the Acting Director of Agriculture (Mr. E. Rodrigo), the Chairman, Planters' Association of Ceylon (Mr. R. C. Scott), the Chairman, Ceylon Estates Proprietary Association (Mr. D. T. Richards), Major J. W. Oldfield, C.M.G., O.B.E., M.C., Messrs. C. E. Hawes, R. G. Coombe, J. D. Hoare, T. B. Panabokke, Col. T. G. W. Jayewardene, V.D., and Dr. R. V. Norris (Director, T. R. I. and Secretary).

Mr. J. C. Kelly had intimated his inability to be present.

1. The Notice calling the meeting was read.

2. The Minutes of the Meeting of the Board held on the 23rd December, 1937 were confirmed.

3. MEMBERSHIP OF THE BOARD AND COMMITTEES.

(a) Reported that Mr. C. E. Hawes had been nominated by the C. E. P. A. to act, with effect from 16th March, 1938, for Mr. I. L. Cameron who had proceeded on sick leave, *vide* letter dated 18th March, 1938 from the Secretary, C. E. P. A.

(b) Reported that Mr. R. P. Gaddum vacated his membership of the Board and of the Finance Sub-Committee on 18th March, 1938, on relinquishing the Chairmanship of the Planters' Association of Ceylon, and was succeeded by Mr. R. C. Scott.

(c) Reported that Mr. R. G. Coombe had been renominated as a member of the Board by the Planters' Association of Ceylon for a further period of three years dating from 1st January, 1938, *vide* letter dated 3rd February, 1938 from the Secretary, Planters' Association of Ceylon.

The Chairman welcomed Mr. Hawes and Mr. Scott, and referred to the excellent work Mr. Gaddum had carried out for the Institute while serving on the Board.

4. FINANCE.

(a) *Auditors' Report on the Institute and Estate Accounts for 1937.*—The Chairman reported that the Auditors' Reports on the Accounts for 1937 had been circulated to the Finance Committee and had been considered by that Committee that morning. The reports on both the Institute's general accounts and the estate accounts were very satisfactory indeed.

(b) *Institute's Accounts to 28th February, 1938.*—On the proposal of Mr. R. G. Coombe, seconded by Mr. J. D. Hoare, the Institute's accounts to 28th February, 1938 were approved without comment.

(c) *Tea Research Institute Cess.*—Reported that the Ceylon Estates Proprietary Association and the Planters' Association of Ceylon had both unanimously supported the Institute's proposal that the Cess should be maintained at 14 cents per 100 lbs. until December 31st, 1943.

The Chairman said that the proposal would now be forwarded to the Ceylon Association in London and the L. C. P. A. and he hoped the matter would come before the State Council about July.

(d) *Resthouse.*—The plans and estimates of the proposed Resthouse were tabled, and with certain minor modifications were approved.

It was also agreed that the Finance Committee should draw up rules and scales of charges for the use of the Resthouse.

5. SENIOR SCIENTIFIC STAFF.

ENTOMOLOGIST.

i. Reported that by circulation of papers (Circular No. B. 1/38 dated 4th March, 1938), Mr. King had been granted 8 months' leave as from 13th April, 1938, and that Mr. G. D. Austin had been appointed to act as Entomologist.

ii. The Board approved of a grant to Mr. King towards expenses to be incurred in attending an International Entomological Conference in Berlin in August, 1938.

6. JUNIOR SCIENTIFIC STAFF.

Reported that Mr. G. D. Austin returned from sick leave on February 19th, 1938, and that he had visited various Entomological Laboratories in England and abroad while on leave.

7. ST. COOMBS ESTATE.

(a) Visiting Agent's Report dated 20th December, 1937:—

Reported (i) The labour position is now satisfactory.

(ii) *Tephrosia vogelii* is to be replaced by Crotalaria as a temporary measure in view of the spread of eelworm.

(iii) The estate block in the weeding trial has been cleared, leaving three or four species of cover only. The experimental weeding plots are being continued.

(b) *Estate Furniture.*—The Board sanctioned expenditure of Rs. 70 for replacement of articles written off in the Teamaker's and Estate Clerk's quarters.

(c) *Factory Chimney.*—The Board sanctioned expenditure of Rs. 400 for replacement of the chimney to the E.C.P. Drier.

(d) *New Clearings.*—Reported that the Experimental and Estate Committee had decided to open the commercial area of the 1938 clearing on a modified contour system, planting being 4 feet $\times$ 2 feet.

(e) *Midwife.*—Reported it had been found impracticable to share a midwife with Kowlahena and that enquiries were now to be made regarding a joint scheme with Mattakelle.

(f) *Water Service.*—Reported that the pump and motor of the main water service would require replacement at an early date. Detailed estimates had not yet been received, but approximate figures would be Rs. 600 for the pump and Rs. 350 for the motor. The Director was authorised to proceed with the work.

8. MINUTES OF THE 36TH MEETING OF THE EXPERIMENTAL AND ESTATE SUB-COMMITTEE HELD ON 22ND JANY. 1938.

Packing Materials.—The Director reported that the Tea Technologist had for a considerable period been experimenting on various types of packing materials as an alternative to metal foil. A material known as Kraft Paper had proved highly satisfactory. Unfortunately as this material had no second-hand value, buyers both in Colombo and London were unwilling to support the use of this lining.

Mr. Richards said in regard to packing materials the position was no doubt disappointing to the Institute. He did not think the Associations concerned could do more in the matter, and it was really for the firms interested in such packing materials to take steps to convince buyers of the advantages to be gained by their use.

9, EXPERIMENTAL WORK AND STAFF AT SUB-STATIONS.

The Director said that, as indicated at the last meeting, the field experiments which had been in progress at Galatura Estate would terminate early in May. The Compost Experiment at Wikiliya would, however, be continued. Up to the present time this experiment had been supervised by the Plant Physiologist's Field Assistant at Galatura. The time of this assistant would, however, now be required for work on tea selection and propagation. In these circumstances other arrangements would have to be made.

Allowance had been made in the draft estimates for the engagement of an additional Field Assistant in 1939, and he asked that this appointment might instead be made now. The Assistant would work under Dr. Eden and be stationed at Passara where accommodation could be rented. He would visit Wikiliya as required.

Mr. R. G. Coombe asked how often the assistant would visit Wikiliya and how his travelling expenses would be met.

In reply the Director said weekly visits would probably be required to Wikiliya, the expenses being met from the Field Experiment Vote. The time of the assistant would, therefore, be divided between the Wikiliya experiments and Dr. Eden's field experiments at Passara.

The Board approved the Director's proposals and sanctioned the appointment.

10, COMBINED WORK ON THE CHEMISTRY OF TEA.

The Director reported that Dr. Lampitt had agreed to act as advisor to the Board on this question. Details of the work required had recently been discussed with Mr. Carpenter, the Chief Scientific Officer at Tocklai, who was now on his way home, and Mr. Carpenter would take up the matter with the Indian Tea Association as soon as he reached London.

11, DISEASES AND PESTS.

(a) *Eelworm.*—The Director reported that a case of eelworm attack in *mature* tea had recently been noticed. In reply to Mr. Hoare he said the estate in question was at a very high elevation.

(b) *Shot-hole Borer.*—The Chairman said the possibility of obtaining certain predators and parasites against Shot-hole Borer had recently been discussed with the Farnham Royal Laboratory and certain species had been suggested for trial. He felt the Board would willingly sanction the expenditure involved in collecting these which might be some £25.

The Director said it was more than problematical whether any success would be achieved. The question was so important, however, that he had already asked the Farnham Royal Laboratory to collect the species in question and send them out for trial. He thought the cost could be met from the existing vote for Miscellaneous Field Experiments.

(c) *Tortrix*.—The Director said considerable success was being obtained with the parasite (*Macrocentrus* sp.) obtained from Java. This appeared to be establishing itself where liberations had been made. Distribution for technical reasons would be rather slow but further liberations would be made in the autumn.

The Chairman said this parasite had been obtained as a result of the personal contacts established during the visit he and the Director had paid to Java.

12. ANY OTHER BUSINESS

(a) *Small-Holdings*.—Reported that a very successful competition had recently been completed in the Yati-Nuwara area. Dr. Eden who had carried out the final judging was very favourably impressed with the work carried out.

On behalf of the Board the Chairman proposed a hearty vote of thanks to all those who had co-operated in the competition.

The Director mentioned that all the prize money required for the competition (Rs. 150) had been contributed by the Village Committees in the area.

(b) Reported that Mr. P. H. Carpenter, Chief Scientific Officer, Tocklai, had spent three days at St. Coombs in March when the research programmes at St. Coombs and Tocklai were discussed.

Sir Frank Stockdale had also visited the Institute on the 7th April.

(c) *Annual Report*.—Reported that the draft Annual Report of the Board for 1937 had been approved by the Board. (Circular No. A.7/38).

The meeting then concluded with a vote of thanks to the Chair.

ROLAND V. NORRIS,
Secretary.

NOTICES.

The Institute's Laboratories and Offices are situated at St. Coombs, Talawakelle, and all applications and enquiries should be addressed to the Director, Tea Research Institute, St. Coombs, Talawakelle.

Specimens and other consignments sent by rail should be forwarded to Talawakelle Station c/o Messrs. Hemachandra & Co., Forwarding Agents.

Visitors' Day.—The *last* Wednesday in each month has been set aside as Visitors' Day at St. Coombs Estate and also at the T. R. I. Sub-Station, Gonakelle Estate, Passara, when it is hoped anyone interested will visit the Stations.

Visitors at other times are welcomed, but it is requested that an appointment be made if possible.

ROLAND V. NORRIS,
Director.

The Tea Research Institute of Ceylon.

BOARD OF CONTROL

(A) Representing the Planters' Association of Ceylon :—

(1) Mr. R. G. Coombe

(2) Mr. Jas. Forbes (Jnr.), (Chairman)

(3) Mr. J. D. Hoare

(B) Representing the Ceylon Estates Proprietary Association :—

(4) Major J. W. Oldfield, M.C., C.M.G., O.B.E.

(5) Mr. I. L. Cameron (on leave) Mr. C. E. Hawes (acting).

(6) Mr. J. C. Kelly (on leave) Mr. K. W. Taylor (acting).

(C) Representing the Low-Country Products' Association :—

(7) Colonel T. G. W. Jayewardene, V.D.

(D) Representing the Small-Holders :—

(8) Mr. T. B. Panabokke

(E) Ex-Officio Members :—

(9) The Hon. the Financial Secretary.

(10) The Director of Agriculture

(11) The Chairman, Planters' Association of Ceylon

(12) The Chairman, Ceylon Estates Proprietary Association

Secretary, Roland V. Norris, D.Sc., St. Coombs, Talawakelle.

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The publications of the Tea Research Institute will be sent, free of charge, to Superintendents of Ceylon tea estates, over 10 acres in extent, and to Estate Agencies dealing with Ceylon tea, if they register their names and addresses with the *Director, Tea Research Institute of Ceylon, St. Coombs, Talawakelle.*

Other persons can obtain the publications of the Institute on application to the Director, the subscription being Rupees fifteen per annum for persons resident in Ceylon or India, and £1-5-0 for those resident elsewhere. Single numbers of *The Tea Quarterly* can be obtained for Rs. 2-50 or 4*s*. In the case of Indian cheques four annas should be added to cover commission.